BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded in 1887
by
G. STANLEY HALL

VOL. XXXIII APRIL, 1922 No. 2

AN EXPERIMENTAL STUDY OF CHILDREN AS OBSERVERS[1]

By CLAIRE COMSTOCK and HELEN KITTREDGE

The current text-books of child psychology seem to be made up of (1) a summary of observations, often of the most casual sort, taken by differently trained observers on different individual children (*i. e.*, varying as to age, sex, heredity, environment, etc.) and under quite different conditions, (2) a presentation of statistical results of studies made of groups of children in classrooms, including mental tests; and, finally, (3) that admixture of sentimentality without which no text-book of child-psychology appears to be complete, but which would be ridiculed in a treatise of general psychology.

Much of the work has been done with single children, since the emphasis has been largely on the development of the child-mind, and it has accordingly been thought better to observe one child carefully from birth to early adolescence than to work with numbers of children. These studies of individual children[2] are interesting and valuable to child-psychology. They seem, however, to be but the beginnings of a child-psychology. Different investigators reach different conclusions and, at best, the number of reliable studies of this sort is small. Group-studies have concerned themselves, for the most part, with investigations of the "learning process" and mental tests, and have had pedagogical applications as their goal. Few of the studies are comparable with the experimental studies of the "adult mind," and therefore the text-book of child-psychology

[1]From the Psychological Laboratory of Smith College.

[2]Cf. W. Preyer, *The Mind of the Child*, 1890; M. Shinn, *The Biography of a Baby*, 1900.

has not as a basis the results of experimental work which make possible the definite statements of fact in general psychology.

One reason for this difference is the tacit assumption that the child cannot describe his experiences, and that he cannot and should not be brought into a laboratory where the conditions of experiment can be controlled and repeated under exactly similar conditions with other children. Many writers declare that the atmosphere of a laboratory is unnatural and that the child brought there becomes, for the time, an unnatural being. The first objection—that children can not describe their experiences—seems more serious. But Binet in 1903 writes of his work with Armande and Marguerite: "Les recherches que j'ai pu faire sur ces deux enfants sont extrêmement nombreuseset se sont espacées sur trois ans. Elles s'y ont prêtées avec beaucoup de bonne grâce, sans timidité, ni fou rire; elles ont toujours compris qu'il s'agissait d'une chose sérieuse, et elles étaient persuadées que le moindre erreur pouvait me causer un préjudice des plus graves. Plût au ciel que les adultes qui servent de sujets aux psychologues eussent toujours une attitude aussi bonne!"[3] At the close of the book he says further: "J'ai été aidé dans cette oeuvre difficile (la nature de la pensée) par ces deux enfants qui ne savent pas le premier mot de psychologie."[4] More recently E. R. Jaensch and his students [5] report the obsertions of children, 11 to 17 years of age, in experimental investigations.

The experimental work about to be described has had an aim somewhat different from those governing the work of Binet and Jaensch. We have been interested in the ability of children to make psychological observations rather than in the study of any bit of experience for itself. Our work has been in the field of visual sensation; but our aim has been, primarily, not to contribute any new data to the child-psychology of visual sensation, but to find out how accurate and reliable descriptions of experiences a child can give in a laboratory under controlled conditions. We have used experiments in visual sensation because this field has been so well worked over that we had furnished a basis for comparison of the observations made by children with those made by adults. Our problem has been largely one of method.

There were eight *Os*: M. (4 yrs. 8 mos.), B. (5 yrs. 6 mos.), K (9 yrs.), J. (10 yrs. 6 mos.), I, (11 yrs.), F. H. (11 yrs.), F. B. (17 yrs. 5 mos.), and A. (20 yrs.).[6] F. B., a freshman in college,

[3] A. Binet, *L'étude expérimentale de l'intelligence*, 1903, 10.

[4] *Ibid.*, 308.

[5] *Zeits. f. Psych.*, 84, 1920, 1ff.; 85, 1920, 37ff.

[6] B. and K. were boys; the others, girls. M. and B. were children whose mothers worked as maids in the college houses and who, therefore, had a poorer home environment than the other *Os*.

and A., a junior, were chosen as older *O*s in order that we might make some comparison of the character of their reports and those of the six children. None of the *O*s had had any previous experience in a psychological laboratory, but A. had taken a one-semester course in introductory psychology.

J. and F. B. came to the laboratory one hour a week for 21 consecutive weeks with the exception of the college vacation periods. The number of weeks devoted to experimental work with the others was shorter, owing to delays in starting or to interruptions. F. H. was unable to come to the laboratory after the first 11 weeks; and the fact that B. was ill for 6 weeks caused an interruption in his observations. We did not start work with A., I., K., and M. until after we had been working with the others for several weeks. With K., especially, the work was crowded and incomplete. M. observed in only 7 parts of the experiment, and her observation-periods were often shorter than an hour.

Because of the age of the *O*s it was necessary to give the instructions in as simple language as possible and to avoid all suggestion. Absolutely no conversation, after the giving of the instructions, took place between *E* and the *O*s. All of the experimental work was done in the psychological laboratory. We endeavored to make the work interesting, and the children apparently liked to come. We found it impossible to get reports on a single phase of an experiment for a whole hour, for the children tired easily. We feel quite definitely that shorter observation-periods would be better; but it seemed impossible to arrange for a number of shorter periods, so that we had to make the most of the one hour a week that the children gave us. To obviate fatigue and loss of interest, we varied our procedure during the hour of observation, securing, for example, reports upon the after-image both in the dark room and outside.

Experiment A

We began the experimental work with a study of the after-image, since here we had an objective check on the experiences of the *O*. It should be noted, however, that this is not an obvious observation, for many psychologically untrained adults fail to observe the after-image even when their attention has been called to the phenomenon. Our instructions carefully avoided all such suggestion.

We attempted to verify the following laws of the after-image. (1) The color of the image is always antagonistic to the color of the stimulus. (2) A contrast-color in the stimulus is effective in the after-image. (3) The after-image is intermittent or periodic, not continuous. (4) The intensity of the after-image is a function of the intensity of the stimulus.

Procedure.—(1) In the verification of Law 1 we used adaptation-cards (18x24 in.) of different shapes: (*a*) oblong, half-black, half-white, with a small circle half-white, half-black, which served as a fixation-point, slightly above the middle of the median line; (*b*) circle, half-white, half-black; (*c*) diamond, half-black, half-white. Each card was exposed to the *O* for 30 sec. The instructions were: "I want you to look at this cardboard about here (a point slightly above the middle of the median line, indicated by *E*) until I put the gray cardboard over it like this. Then look at the cardboard and tell me what you see; tell me all about it."

TABLE I

After-Image Reports

	Without Training					With Training					
Os	No. Trials	A.-I.	Qual.	Period	Mvt.	Weeks Train.	No. Trials	A.-I.	Qual.	Period	Mvt.
M	7	0	0	0	0	—	—	—	—	—	—
B	4	0	0	0	0	26	8	8	6	0	0
K	8	7	7	1	1	9	3	3	3	1	0
J	13	13	13	0	4	23	8	8	8	0	4
I	11	11	11	0	1	12	8	8	8	1	0
FH	13	13	13	0	2	—	—	—	—	—	—
FB	11	11	11	2	0	22	8	8	8	0	0
A	8	8	8	0	1	9	8	8	8	0	0

Results.—The above table indicates the number of trials for every *O*, the number of times an after-image, the correct quality, a periodicity, and a movement were reported spontaneously.

M. and B., the two youngest *O*s, gave no reports, declaring that they "saw nothing." All of the other *O*s described the after-image. It should be remembered that we did not ask for a report on any one aspect of the after-image, so that all descriptions were spontaneously given. Our procedure throughout the work has been to try for a spontaneous report of a given phenomenon and, having obtained it, to ask for it directly in the instructions of the next experiment. We hoped by this procedure to avoid all suggestion.

Samples of the *O*s' reports follow. In view of the purpose of this experiment they seem to the *E*s quite as important as the statistical results. They show the naiveté and the good faith of the *O*s, and give some indication of the difficulties of procedure where so young *O*s are used.

Samples of Reports. The numbers in parentheses indicate the number of the trial for which the report was given.

K. (2) "White on this side and black on that. How did it come through? Saw it, and then when I was telling you I saw it again."

J. (1) "One half of the page on the right side like a white piece of paper and like a little hollow in half of it."

I. (4) "I have the feeling something funny is coming with this. It is a circle like the other one, only turned around."

F. H. (2) "Side that was white is black and side that was black is white."

F. B. (4) "White on right, black on left; center circle white on left, black on right."

A. (7) "Black on this side, half circle of black sticking out; white on that side and half circle."

After an interval of several weeks, the length of which differed with the different *O*s, we repeated this part of the experiment with the same instructions to see the nature of the effect of intervening practice upon the type of report. The results are given in Table I. Lack of time prevented repetition of this experiment with M, but in the case of B, the next youngest *O*, the period of training in observation produced favorable results.

In order to secure a report of periodicity the following addition to the instructions was made: "Now this time I want you to raise your hand when what you see is gone." Periodicity was reported by all the *O*s, except M. and B., who "saw" no after-image.

Samples of Reports

J. (1) "Moves, comes back.....now it's gone altogether."
F. H. (3) "It's all gone.....no, it isn't."

(2) When the *O*s had shown their ability to name colors, the second part of the experiment was conducted in the dark room, in order that there might be less opportunity for distraction. In a square opening 3x3 in. in the end of an oblong-shaped box, were placed in turn red, yellow and green glasses. A square of white ground-glass was placed behind the colored glass, and an electric light was inserted into the back of the box through a small opening in the top. In a few experiments outlined apple and diamond shapes were placed behind the colored glasses. The exposure-time was 30 sec. The instructions were: "I'm going to show you a color and I want you to look at it until I put the card over it. Then look at the card and tell me what you see; tell me all about it." The number of trials in each case was determined by the nature of the report.

TABLE II

	No. of Trials			After-Image			Quality			Contrast-Effect		
	R	Y	Gn	R	Y	Gn	R	Y	Gn	R	Y	Gn
M	14	12	10	1	1	1	1	1	1	1	0	0
B	14	12	9	9	6	5	6	3	0	3	0	1
K	7	5	6	7	4	5	7	4	5	1	1	0
J	5	5	6	5	5	5	5	5	5	2	5	5
I	5	11	7	5	10	7	5	1	2	0	1	1
FH	2	2	4	2	2	4	2	2	4	0	0	0
FB	4	4	6	4	4	6	4	4	6	0	0	1
A	3	3	3	3	3	3	3	3	3	1	2	0

Results.—As in Table I, we have given the number of trials for every *O*, the number of times an after-image and the correct quality were reported, and in addition the description of a contrast-effect in the stimulus or in the after-image. For the first 6 trials with each stimulus color M gave no report. After this, with 3 exceptions noted above, she always reported "black." This "black" may have been the brightness-aspect of the after-image or merely the blackness of the dark room; the former seems the more probable hypothesis, since when M "saw" nothing, she gave no report at all. B. showed considerable improvement and the other *O*s, as the samples of reports will show, gave more accurate descriptions.

Samples of Reports

M. (7) "Blue and black." (Stim. yellow.)

B. (7) "Don't see nuthin, don't see nuthin', don't see nuthin'. I see green, green, green." (Stim. red.)

K. (3) "Green. I close my eyes and I see green." (Stim. red.)

J. (2) "Kind of a green square with a red line around it, moving around." (Stim. red.)

I. (2) "Biggest green square, keeps coming and going like a flashlight. When I took my eyes off of the red everything went green." (Stim. red.)

F. H. (1) "Oh, oh! I see green with light yellow around it, pretty." (Stim. red.)

F. B. (4) "I see at first lightish blue-green and then it changes into dark green. Outside of it is a circle of pale yellowish light, then just blue green square, then nothing." (Stim. red.)

A. (1) "Purple, sort of bluish-purple, with yellow haze around it..... it gets darker." (Stim. yellow.)

(3) After several trials with every stimulus the additional instruction was given: "Now this time I want you to say 'go' when what you see has gone. Say 'come' when it come sand 'go' when it has gone."

Results.—All of the *O*s except M, who did not observe in this part of the experiment, reported not only the first and the last appearances of the after-image, but noted several reappearances as well.

Now that the *O*s had spontaneously noted the periodicity of the after-image, the instructions were: "You saw that the color went and then came back. Now this time I want to know how many times the color comes and goes. When you first see it say 'come' and if it goes say 'go,' and so on. In short, every time you see the color say 'come' and every time it goes say 'go'." There were at least 3 exposures of the red stimulus and 3 of the green.

Results.—All of the *O*s (except M) reported recurrences of the after-image, varying in number from one to seven. The two older *O*s were more regular in their reports of periodicity, but otherwise there was little difference between the reports of the older and the younger *O*s.

(4) We asked next for a description of the after-image with eyes closed. The instructions were given as in A 2, with this change; instead of: "Then look at the card and tell me what you see," the *O*s were told: "Then shut your eyes and tell me what you see."

Results.—All of the *O*s described correctly the quality of the after-image with the exception of the after-image with the yellow stimulus. B. called this "black", and I. gave no report whatsoever. It is worthy of note that the *O*s now report not only quality but also shape, and frequently contrast-effect, in the after-image. The report given by B., cited below, is the first of a number of this kind.

Samples of Reports

B. (1) "Green mark, yellow mark, green tree, green house, green river, green sidewalk, people swimming around the edge." (Stim. red.)
K. (1) "I know what it's going to be.....see it on the floor now. Square of blue." (Stim. yellow.)
J. (1) "Purple square with green around it." (Stim. green.)
I. (1) "I see something funny.....a blue square and a green square." (Stim. red.)
F. H. (1) "Cerise-purple, it's dandy." (Stim. green.)
F. B. (1) "Purple square with pale circle of light outside." (Stim. green.)
A. (1) "Bluish-purple square with light yellow haze around it. Doesn't come as quickly with eyes shut as with the card." (Stim. yellow.)

(5) In order to obtain a description of contrast-effect in both stimulus and after-image the stimuli were exposed for 45 sec. each. The instructions were: "Tell me what you're looking at; tell me all about it"; and when the *O* had reported contrast in the stimulus, "Just tell me about the color." Then followed the after-image contrast instructions: "When I put the card over this I want you to tell me just about the color you see."

Results.—This proved to be a difficult observation to make. We were, perhaps, over-cautious in the phrasing of the instructions, which certainly gave the *O*s no clue as to what we were after, and which may have developed a set against a report of contrast because of similarity to the instructions used in A 1 and 2. However, all of the *O*s (B's report is questionable; but it seems probable that he observed a contrast-effect in the stimulus) described a contrast-effect in the stimulus; but only one of the *O*s, J., always observed a contrast-effect in the after-image, though F. H. described it once out of twelve trials. J. was the best of the younger *O*s. She came the most regularly to the laboratory, so that she had more practice than the others.

Samples of Reports

B. (4) "An automobile with green." (Stim. red.)
K. (1) "Blue right under it." (Stim. yellow.)
J. (1) "Dark around square, green around it." (Stim. red.)

I. (1) "Dark spot in the middle, light green off to the side." (Stim. red.)

F. H. (1) "Green getting around it; red with green rim around it." (Stim. red.)

F. B. (1) "Blue-purple around outside." (Stim. green.)

A. (5) "Green square. Right now it is getting lighter in color, more yellowish, except that now I see the negative after-image of dark reddish purple covering part of it, and that makes the center look like dark blue-green. It seemed to be getting darker and then all of a sudden I realized that it was a darker square getting over it." (Stim. green.)

(6) To obtain a description of differences in intensity, we covered half of each of the glass squares with tissue paper, so that the one half of the square appeared much brighter than the other. The instructions were: "I want you to look at this until I put the card over it; then look at the card and tell me what you see; tell me all about it." The exposure time was 30 sec.

Results.—B., K. and I., after several trials, gave no reports of differences in brightness. J., F. H., F. B., and A. gave satisfactory reports.

Samples of Reports

J. (1) "Kind of purplish; half light purple and half blue, yellow around it." (Stim. yellow.)

F. H. (1) "Where green was, it's purple; where it had white paper on it, very light." (Stim. green.)

F. B. (4) "Pale green on the left, darker blue-green on the right." (Stim. red.)

A. (1) "Purple on one side.....can't describe the shade on the other .....it's more brownish and lighter." (Stim. green.)

(7) During the course of the experimentation in the dark room, we asked the *O*s to describe the after-image from an electric light. The instructions were: "I'm going to pull the light on and off three times. When I say 'now' I want you to tell me what you see in the darkness; tell me how the light looks in the darkness."

Results.—All of the *O*s described both the positive and negative after-images, though the reports varied in exactness and in the number of colors described. The youngest *O*, M., in the twenty-third trial gave the following sequence of colors: "Grey, yellow, red, blue, green." Samples of reports from the other *O*s follow.

Samples of Reports

J. (4) "Yellow strings, then lavender, then yellow."

B. (1) "Red coming down, red coming down, red coming down, yet down like a fire."

K. (3) "Those little wires I see..... a little green over those wires."

I. (1) "Big round circle, light in the middle of it, like a lamp hanging up high."

A. (6) "Yellow, shape of bulb.....I think I see some blue."

At the conclusion of the whole of our experimental work we repeated this part of the experiment. The reports were all more detailed, *i. e.*, more colors were observed.

J. (3) "The wires are first red, then they turned lavender and then purple, and then black."

(8) This part of the experiment we conducted under ordinary daylight conditions. A brief explanation of the term "after-image" was given to the *O*s.

(*a*) Small red, green, yellow and blue squares of paper about 4x4 in. were fastened on gray backgrounds. Each was exposed for 30 sec. The instructions were: "I want you to look at the color until I put the card (gray) over it; then look at the card and tell me all about the after-image."

Results.—All of the *O*s, except M., who failed to observe the negative after-image, described correctly the quality of the after-image, and reported spontaneously stimulus-contrast, movement, and periodicity. The older *O*s named the colors of the negative after-image more accurately, as, for example, "bluish-purple," "turquoise-blue," etc.

(*b*) Small green, blue, and red squares of paper were fastened on gray backgrounds. Blue, red, and green backgrounds were used for the projection of the after-image. The instructions were the same as in (*a*).

TABLE III

	Stim.	Bkgd.	Stim.	Bkgd.	Stim.	Bkgd.	Stim.	Bkgd.
	Green	Blue	Blue	Red	Blue	Green	Red	Green
O	Trial	Report	Trial	Report	Trial	Report	Trial	Report
M	—	—	—	—	—	—	—	—
B	1	blue*	1	yel., red	1	—	1	green, blue*
	2	blue, green	2	yel. marks	2	green*	2	—
	3	blue	3	blue, red	3	yel., blue, red		
			4	blue, red	4	yel., green		
K	1	purple†	1	purple†	1	yellow†	1	blue
			2	orange†			2	blue
J	1	purple*	1	yellow*	1	yellow*	1	—
			2	yellow	2	yellow*‡	2	green*
I	1	purple*	1	scarlet	1	green*†	1	lt. green
			2	yellow†				
			3	scarlet*				
FH	1	cerise	1	orange	1	yel. orange	1	dk. green
					2	orange	2	wh. blue
					3	yellow		
FB	1	purple*	1	orange	1	yellow	1	blue-green
					2	strong yellow		
A	1	purple*†	1	orange*†	1	greenish-yel.*	1	blue-green

*—shape reported
†—periodicity reported
‡—movement reported

Results.—M. observed no after-images. B.'s reports are again questionable. In trial 2 with the green stimulus and the blue background he may, for example, have seen the "correct" purple, which he called "blue," and his "green" may have been the contrast-effect from that. This is mere conjecture, however, and as such worthless. The case cited shows one of our difficulties with the younger *O*s, a just interpretation of reports. With the blue stimulus and the red background in trial 1 his

report of "yellow" is correct; but in the following trials with the same stimulus and the same background there are no similar reports. This variability is another characteristic of young *O*s.

(9) In order to determine the effect of training and also to test the possibility of using written instructions, in a final synthetic experiment we used either purple or blue colored glass shown to the *O* in a manner similar to that described under (2).

(*a*) Before each of the three 45 sec. exposures the following instructions were read: "I'm going to show you a color and I want you to look at it until I put the card over it; then look at the card. When the after-image comes, watch it until it has gone for good, and then tell me all that you can about it. Tell me all the different things that you've been telling me during the last few weeks, but be sure not to tell me any of these things until the after-image has gone for good."

Samples of Reports

B. (6) "Green piece of grass and green lettuce and a white boat and a red boat and that's all." (Stim. purple.)

I. (1) "Green square and it stayed in the middle of the board. When there's purple there's green around it, and when there's green there's purple around it." (Stim. purple.)

F. B. (3) "Pale circle of bluish light with muddy brown yellow. It comes and goes and it decreases each time in brilliancy." (Stim. blue.)

(*b*) At least two more 45 sec. exposures were made, but this time before each the *O*s were asked to read the following typed instructions. (B. could not read the typed instructions so that *E* read them to him.) "I am going to show you a color and I want you to look at it until I put a card over it. Then I want you to look at the card. When the after-image comes, watch it until it has gone for good, then tell me all that you can about:—(1) the stimulus, its color; (2) the after-image, its color, its size and shape, its coming and going."

Samples of Reports

B. "The stimulus is pink. The after-image is green. It comes and goes, is square."

K. "The stimulus is purple and the after-image green. It came and went and was the same size and shape."

I. "On the stimulus is a little light circle. The after-image is a light green that comes and goes. It is a little bit smaller than the stimulus. It was square."

J. "The stimulus was purple. The after-image is a sort of white cloud. I think it went twice."

F. H. "The stimulus was blue, the after-image yellow. It was the same size and shape as the stimulus. It came and went twice."

F. B. "The stimulus was a purplish-blue, the after-image came and went about three times. It stayed longer the first two times. It was a square about the same size and the square was a muddy brown yellow surrounded by a light pale bluish color."

A. "The stimulus was a reddish lavender. It got darker but that was the negative after-image which was not quite on the square but overlapped

it. The after-image came almost immediately; a dark olive-green square the same size as the stimulus. It gradually got darker and faded into the background."

The reports indicate a great improvement over the earlier ones, and show that even very young *O*s can report on a number of details and that written instructions can be successfully used.

(10) Our final experiment in this group was an attempt to see if these young *O*s could compare two after-images with respect to brightness. One 30 sec. exposure was made with the plain blue glass. When the after-image from that had disappeared, another 30 sec. exposure was made of the blue glass covered with thin white tissue paper. The instructions were: "I am going to let you see two after-images, one after the other. Try to remember how the first one looks and, after you have seen the second and it has gone for good, tell me how the two are different from each other."

Results.—This differs from any of the former procedures in that a report can not be "read off" from the immediately present experience. B. and I. reported no difference in brightness, but all the other *O*s noticed and described the change in this aspect.

Summary of Results and Conclusions

(1) All of the *O*s reported the presence of the after-image, positive and negative, and described correctly its quality, at some time during the experiment. With the exception of M., the youngest, all described the intermittent character of the after-image and a contrast-effect in the stimulus. The relation between the duration of the stimulus and the duration of the after-image was observed by the older *O*s, I., F. H., and A.; the effect of contrast in the after-image by J. and F. H.; differences in brightness by J., F. H., F. B., and A.

(2) The *O*s improve with training. B. and M., the youngest, "saw" no after-image at first, but after training described it correctly. B. gave no reports of any sort during the first 2 hours that he spent in the laboratory, so that we felt that we had reached the lower age limit in the *O* of five and one half years. We decided, however, to "try again," and in the next observation hour B. gave accurate reports, and so improved with training that he frequently matched the older *O*s. We had much the same sort of experience with M. She was still younger than B., but like B., lacked the better training and home environment that the other *O*s had. We worked with M. for only 6 hours, and her reports are inaccurate. However, they show a progressive improvement, and point to the conclusion that the ability to describe one's experiences is a matter of training rather than of age, except so far as the understanding and use of language are correlated with age.

The older *O*s improve with training in accuracy of report and in the number of details they give. J. was the best of the younger *O*s, but she came the most regularly and for the longest period of time to the laboratory. Training, then, is very important. It involves not only improvement in the use and understanding of the language of description, but also an adaptation to the atmosphere and requirements of laboratory work.

(3) The greatest difference between the younger *O*s and the older is the lack of stability of judgment on the part of the younger ones; that is, when an *O* gave a correct report, this was no guarantee that he would continue to do so. A careful description was quite likely to be followed by one, or several, which seemed to have nothing to do with the experience; or by no report at all. This was especially true of M. and B. and, to some extent, of K. and I., though their stability of judgment improved with training. It seems probable that this variability shown by the youngest *O*s was correlated to some extent, at least, with fatigue and ennui. Shorter observation periods might not eliminate it, but they would help to decrease it. J. and F. H. differed very little in this respect from the older *O*s, F. B. and A. Indeed, toward the close of the experimental work, we hardly looked upon J. as a "younger *O*". K. and I. had so much shorter periods of training than B., J., and F. H. that their reports are not strictly comparable.

(4) The reader will have noticed the naiveté and, in some cases, the crudity of the reports. The younger *O*s obviously know only the more simple color-names. We have not had many difficulties in interpreting the reports. They are more comprehensible because of their frankness than many reports of older *O*s in other experimental work. B.'s reports are an exception to this statement. His descriptions of "grass," "lettuce," and "boats" are not easily interpreted. How much of the imagery is based on directly preceding sensory conditions and how much of it is supplementary imagery cannot be said. *E* at no time criticized B. for this type of report or commented on it, except by saying at the beginning of the experiment: "Now tell me *just* what you see."

Experiment B

Since the *O*s had improved with training, we continued our work in the field of visual sensation and took up next a study of *adaptation*. Observations in this field proved difficult for young *O*s because of the constant fixation of considerable duration (for children) which is necessary, and the slowness with which adaptation takes place, resulting in discouragement and monotony. We spent only from 2 to 3 observation-periods on this part of the experiment, so that the *O*s had comparatively little

training. Several of the children complained that the work hurt their eyes, so that it seemed wise not to give more time to it.

Procedure.—(1) The *O* was seated facing a large window. Directly in front of him was placed a square of green glass, arranged in such a way that all lateral light was excluded. The instructions were: (*a*) "I want you to look through the green glass and tell me about the color of things. Keep looking and tell me how the color changes. (*b*) When you look away, tell me how the things which you see look."

Samples of Reports

M. (2*a*) "The sky is green, the road green, everything green and green and green" (at end of 3 min.).
(2*b*) "Everything is white,.....no, red."

B. (1*a*) "Everything is green.....white.....the church is pink.....road green.....getting black" (at end of 2 min. 20 sec.).
(1*b*) "When you look off things look pink."

K. (1*a*) "Everything is just green.....darker green.....lighter green" (at end of 3 min.).
(1*b*) "Pink.....oh, awful pink."

J. (1*a*) "The sky is green, the automobiles look green, the church green," etc. (at end of 6 min.).
(1*b*) "Things look sort of pinkish.....you look sort of pink..... whiter than you did."

I. (1*a*) "Green.....everything yellow.....now green again..... the church is a horrid olive-green color.....the trees purplish, the sky bright green" (at end of 2 min. 30 sec.).
(1*b*) "Everything bright red.....American Beauty color."

F. B. (2*a*) "Green.....grayish-yellowish green.....gets grayer" (at end of 45 sec.).
(2*b*) "Pink at first, changes back quickly."

A. (1*a*) "Everything is green.....everything a little duller..... doesn't really look green to me now.....I don't know what color, just natural.....a little grayer.....even the grass looks gray" (at end of 2 min. 20 sec.).
(1*b*) "Red! Just as red as it can be."

(2) In order to obtain a report on adaptation to light we used the following instructions: "I'm going to take you into the dark room and we are going to stay there for a short time. (*a*) When we first go in tell me how it looks and, as we stay in there, tell me how it changes. (*b*) When we come out into the light, tell me how it looks and how it changes."

Adaptation to darkness was reported by all *O*s except B. M. did not observe in this part of the experiment.

Samples of Reports

B. (*a*) "Dark.....green, red.....green, blue.....and a dog and a boy.....dark, red, white.....automobile and a blue sky and a stork .....hoop and a boy rolling it", etc. (after 11 min.).
(*b*) "All dark.....now all white."

K. (*a*) "Black.....red, dark.....red now, it was pinkish last time .....pink.....look at my knee.....black still" (after 7 min.).
(*b*) No report.

J. (*a*) "Black.....can't see anything.....all dark.....trying to see something.....all dark.....I begin to see a little better.....can't hardly see anything though.....begin to see things.....every minute or so I see a few more things" (after 15 min.).

(*b*) "Very bright.....not so bright."

I. (*a*) "Can't see anything.....black.....right in front of my eye it's lighter.....seems darker than when I first came in.....light circles twirling before my eyes.....light near the table" (after 8 min.).

(*b*) "Bright!"

F. B. (*a*) "Black.....little gray spots float around.....black.....it's browner than it was, not so deep a black.....the blackness doesn't hit you as it did when you came in. It is not as hard to sit with your eyes open.....looking straight ahead there is grey, black from the sides..... white spots and gray mixed" (after 11 min.).

(*b*) "Everything looks natural."

A. (*a*) "Just dark.....I can't see a thing.....little lighter..... no objects but gray.....perhaps a lighter gray..... a little light streak .....quite a bit" (after 17 min.).

(*b*) "Light, very bright, almost dazzling."

(3) The *O* was seated with his head secured in a head-rest. Five feet in front of him was placed a yellow card with a fixation point. The instructions were: "I want you to sit here with your head in the head rest like this. Then I want you to look at this card about here. As you look at it, tell me what you see, tell me all about it. Keep on looking at it and keep on telling me what you're seeing. (After *O* had reported a change these additional instructions were given.) You saw there was a change in the color. Now just tell me about the changes as they occur." The time of exposure varied according to the nature of the report.

Samples of Reports

B. (2) "Red and a mark and a square mark.....gray.....a round hoop.....white and blue and that's all.....looks blue yet."

J. (2) "Yellow.....light streaks around the sides, top and bottom.....light streaks around the sides or else the middle is darker, I don't know which....almost all the card is a darker yellow, but around the edges it gets lighter."

I. (1) "Funny dark square.....a darker yellow except on the edges where it's the color that it really is.....much darker.....half dark, half light."

F. B. (1) "Yellow.....a brighter yellow around the edge and brownish in the center.....if anything, it becomes more brown."

A. (1) "Light around the spot (fixation point).....a little darker, the whole thing is a very dirty yellow now.....it is almost the same color as the table.....I can't tell where it leaves off.....very grayish."

Summary of Results.—The *O*s clearly need more training in describing the course of adaptation. Some of the reports, however, are good. In B, 1 and 2, all of the *O*s, except K., noticed the after-effects of adaptation. In B, 3 an especially difficult observation was asked for. The nature of the reports shows that changes due to adaptation were observed, but that the work needs considerable refinement. This part (3) of the experiment was very disagreeable to the children, who grew tired

of "waiting for something to happen" and who could not maintain a steady fixation. All but B. and K., nevertheless, describe some of the effects of adaptation. B.'s tendency to give meaningful descriptions is shown throughout the experiment.

EXPERIMENT C

In the next series of experiments we asked for descriptions of contrast-effect.

Procedure.—(1) On backgrounds of red, green, yellow, blue, white and black papers were pasted figures of gray paper. Both were covered by thin white tissue paper. The instructions were: "I want you to tell me the color of each figure as I show it to you."

Results.—We give in full a table of the results of this experiment in order to show how the reports of the different *O*s compare with one another. It will be seen that the older *O*s got the contrast-effect more quickly, that is, with fewer trials, than the younger; though all, even M. and B., made the observation. Observation of contrast-effect was much easier than observation of adaptation, as is, of course, to be expected.

TABLE IV

	Red	Green	Yellow	Blue	White	Black
M	1 white	1 gray	1 gray	1 gray	1 gray	1 gray
	2 "	2 "	2 "-blue	2 "	2 "	2 "
	3 "	3 "	3 blue	3 "	3 "	3 "
	4 "	4 pink	4 "	4 "	4 "	4 "
	5 "	5 "	5 "	5 "	—	—
	6 "	6 "	6 "	6 yellow	—	—
B	1 white	1 gray	1 gray	1 gray	1 brown	1 gray
	2 gray	2 pink	2 "	2 "	2 "	2 "
	3 "	3 gray	3 "	3 "	3 "	3 "
	4 white	5 pink	4 "	4 "	4 "	4 "
	5 gray	4 "	5 "	5 "	5 "	5 "
	6 white	6 "	6 choc.	6 yellow	6 "	6 "
K	1 white	1 reddish	1 brown	1 white	1 brown	1 white
	2 "	2 brown	2 "	2 yellow	2 "	2 "
	3 green	3 red	3 blue	3 "	3 dk. "	3 "
	4 brown	4 "	4 "	4 "	—	—
	5 green	5 "	5 brown	5 "	—	—
J	1 green	1 reddish	1 blue	1 yellow	1 black	1 gray
	2 "	2 pink	2 "	2 "	2 "	2 "
	3 "	3 "	3 "	3 "	3 "	3 "
	4 "	4 "	4 "	4 "	4 dk. gray	4 lt. gray
I	1 gray	1 pink	1 lt. gray	1 dk. gray	1 dk. gray	1 white
	2 "	2 "	2 "	2 "	2 "	2 lt. gray
	3 "	3 "	3 gray	3 gray	3 gray	3 gray
	4 "	4 gray	4 "	4 "	4 "	4 "
	5 "	5 dk. "	5 dk. "	5 lt. "	5 dk. "	5 lt. "
FB	1 blu.-gy	1 purple	1 blu.-gry	1 yel.-gy	1 gray	1 lt. gy
	2 "	2 "	2 "	2 "	2 "	2 "
	3 blu.-wh	1 lav-gy	3 "	3 "	3 "	3 "
	4 blu.-gy					
	5 green					
A	1 gray	1 red-gy	1 bluish	1 yellow	1 dk. gy	1 lt. gy
	2 greenish	2 reddish	2 blu-gy	2 yel-gy	2 "	2 "
	3 gy-gre	3 red-gy	—	3 yellow	3 "	3 "

(2) In this part of the experiment in connection with reports on contrast-effects we secured some data on color-matching. On 6 paper discs (blue, red, yellow, purple, orange, green) were pasted rings of white paper, one-half in. in width. On these rings were pasted sectors of black paper, so that the ring and disc might be equal in brightness. *O* was given a number of small discs (yellow, orange, green, blue, purple, red, black, white). *E* first placed the green paper disc on the electric color-mixer and, having received a description of the contrast-color of the ring, illustrated to *O* how it was possible to combine some of the smaller discs so that when they were rotated on another color-mixer, placed beside the first, the resulting color should match that of the contrasting ring. Following this explanation the different colored discs were in turn put on the mixer. The instructions were: "You saw how I put some of these colors together so that, when the color-mixer was turned on, the resulting color matched the inside ring. Now I want you to do the same. I shall put different colors on the electric mixer, and I want you, by putting some of these colored discs on this mixer, to get colors which will match the inside rings. You may use as many or as few discs as you like."

Results.—With the exception of M. all the *O*s seemed to have had some idea of the proper manner in which to attack the problem. Although M. reported contrast-effects from all colors except purple, she apparently had no idea how she might match the hue and brightness of the ring by combining certain of the colored discs. B. and K. did not always recognize a contrast-color, but they always attempted to match for brightness. They were at first more perplexed by the problem than the older *O*s and were more easily discouraged when good results were not immediately obtainable. K. occasionally departed from the particular problem in his interest in combining various odd colors to observe results. J. always attempted to match for both hue and brightness. She was very persistent in her efforts to secure satisfactory results. I., like B. and K., attacked the problem blindly at first. She always attempted to match for brightness, but she failed to recognize any contrast-color from the purple. F. B. and A., after a careful analysis of contrast-hue and brightness, always attempted to match for both.

The most interesting feature of this experiment was the care and patience shown by the *O*s in matching the contrast-color. Some of the *O*s changed their combinations from 14 to 23 times before they were satisfied with the matches. This result shows that young *O*s can be depended upon for long and care-requiring pieces of work, provided they understand clearly the problem to be solved.

General Conclusions

The experimental work that we have done with children has shown that they can be used as observers in psychological experiments; that they can give accurate and reliable descriptions of their experiences. Training in laboratory procedure is essential, but this is also the case with adult observers. Short observation-periods to avoid fatigue and monotony are desirable, and they should be made as frequent as possible.

The chief difficulties, and these are not unsurmountable, are (1) the use of language that children can understand, (2) the absolute necessity of framing instructions so as to avoid suggestion.

The conditions of this experimental work have been somewhat crude, but they show that with training more elaborately planned experimental work can be carried out. There is no evidence from our work that the children were "unnatural" in the laboratory. Quite to the contrary, once a child had been in the laboratory, its surroundings were taken for granted, and the reports showed no traces of artificiality. Indeed, the observation periods seemed to be enjoyed.

When we compare the children with the two college girls who observed in the experiments, we find that the children show more variability in their attitude and in their reports. This lack of stability decreased with training, but in our work it always had to be dealt with. There was considerable variation from week to week, so that at times it seemed as if the previous week's training had been of no value. There is no doubt, however, but that the interval between observation-periods was too long, and that more frequent observation-periods would favor stability of attitude. On the other hand, the reports of the children were quite as good in most instances as those of the two older *O*s, if we take into consideration the difference in ability to use language. As we have pointed out, the two older *O*s used more exact color names and more carefully chosen words in describing their experiences.

The children, on their side, were more spontaneous in their reports. There seemed always to be more effort on the part of the older *O*s to give what the experimenter "wanted" than on the part of the children, who burst forth into naïve descriptions of what they "saw" without any thought of its being "right" or "wrong." They tended, too, much less toward interpretations of their reports.

As we have said at the beginning of this paper, our purpose was not to make any contributions to the child-psychology of visual sensation. What we have shown, however, is that a psychology of childhood can be built up experimentally. It will take time and patience and some ingenuity, but the results will be worth the effort.

BINAURAL LOCALIZATION OF TONES AS DEPENDENT UPON DIFFERENCES OF PHASE AND INTENSITY[1]

By H. M. HALVERSON

HISTORICAL SETTING

The reader will find the literature on the binaural localization of sound discussed by Pierce[2] and by Ferree and Collins[3].

In referring to the literature certain terminological usages need to be kept in mind. Hearing or localization is *monaural* if only one ear functions in the process; it is *binaural* if both ears function together. Stumpf's[4] terminology makes a finer distinction. Hearing is *monotic* if only one ear functions in hearing; it is *diotic* if both ears function together in the same manner, *i. e.*, if the same tone or tones are heard by both ears; it is *dichotic* if both ears function simultaneously for different tones, *i. e.*, if different tones or complexes of tones are heard simultaneously by the two ears.

The original problem of binaural hearing has arisen out of the demonstration of the fact that, although there may be monaural localization of sound, binaural localization is more accurate. This fact is brought out in the studies of Angell and Fite,[5] Angell,[6] Starch,[7] and Klemm.[8]

---

[1]From the Psychological Laboratory of Clark University. The experiments herein reported were performed subsequently to certain experiments by the writer at the University of Iowa. The monograph reporting these experiments (*Univ. Iowa Studies in Psychol.*, No. 8, 1921) is now in press and explicit reference can not therefore be made to it.

One general way in which the present studies differ from the Iowa experiments is in the appeal made to introspective control of the image of localization and the process of localizing. It is unfortunate that limits of space prevent the printing of complete introspective data, as well as the historical orientation of the problem. All these matters are presented fully, however, in a bound manuscript report of these studies, which is filed under the same title in the Clark University Library, and can be borrowed under the usual courtesies of library exchange.

[2]A. H. Pierce, *Studies in Space Perception*, 1901.

[3]C. E. Ferree and R. Collins, An Experimental Demonstration of the Binaural Ratio as a Factor in Auditory Localization, *Am. J. Psychol.*, 1911, 22, 250-297.

[4]C. Stumpf, Ueber zusammengesetzte Wellenformen, *Ztsch. f. Psychol.*, 1905, 39, 276.

[5]J. R. Angell and W. Fite, The Monaural Localization of Sound, *Psychol. Rev.*, 1901, 8, 225-246.

[6]Angell, Further Observations in the Monaural Localization of Sound, *ibid.*, 449-458.

[7]D. Starch, Perimetry of the Localization of Sound, *Psychol. Rev. Monog.*, 1905, No. 28, 1-45; 1908, No. 38, 1-55.

[8]O. Klemm, Untersuchungen über die Localisation von Schallreizen, 2 Mitteil., *Psychol. Studien*, 1913, 8, 497-505.

The further problem has consisted in a determination of the nature of the binaural integration that operates in these finer localizations of binaural hearing. Wilson and Myers[9] have sought to explain this integration as peripheral and occurring by way of bone-conduction of the sound from one ear to the other. Others (*e. g.* Stewart[10]) have sometimes made use of this principle. There have, however, been numerous objections to the assumption of the effectiveness of bone-conduction. The reader should in this connection consult Cross and Goodwin,[11] More,[12] Peterson,[13] and Klemm.[14]

Peterson and Klemm are for this and other reasons forced to a theory of central integration, a type of explanation which is further supported by Watt.[15] The necessity for integration at the center is apparent when it is demonstrated that stimulation of the two ears is readily discriminated: Cross and Goodwin,[16] Peterson,[17] and Baley.[18]

Apart from certain earlier suggestions that localization might be dependent upon tactual sensations of the shell and drum of the ear, upon stimulation of the semi-circular canals, or upon "original special differences of the ears," there have been three principal theories of binaural localization.

(1) In the first place there is the theory that the localization of direction depends upon the relative intensive differences of a sound as it reaches the two ears. This theory seeks to express the angle of directional localization as a function of the binaural intensive ratio. The view is held, with various degrees of generality, by Angell and Fite,[19] Angell,[20] Starch,[21] Wilson and Myers,[22] Ferree and Collins,[23] Stewart and Hovda,[24] and Klemm.[25]

(2) The opponent theory seeks to explain binaural localization as a function of the two sounds in their phase-relations as they are presented

---

[9]H. A. Wilson and C. S. Myers, The Influence of Binaural Phase Differences in the Localization of Sound, *Brit. J. Psychol.*, 1908, 2, 362-386.

[10]G. W. Stewart, The Theory of Binaural Beats, *Phys. Rev.*, N. S., 1917, 9, 514-528.

[11]C. R. Cross and H. M. Goodwin, Some Considerations Regarding Helmholtz's Theory of Consonance, *Proc. Am. Acad. Arts & Sci.*, 1891, 27, 1-12.

[12]L. T. More, On the Localization of the Direction of Sound, *Phil. Mag.*, 1909 (6 ser.), 18, 308-319.

[13]J. Peterson, The Nature and Probable Origin of Binaural Beats, *Psychol. Rev.*, 1916, 23, 333-351.

[14]Klemm, *op. cit.*, 4 Mitteil., *Arch. f. d. ges. Psychol.*, 1920, 40, 117-146. Referred to hereafter as "Klemm 4."

[15]H. J. Watt, *The Psychology of Sound*, 1917. A Theory of Binaural Hearing, *Brit. J. Psychol.*, 1920, 11, 163-171.

[16]*Op. cit.*

[17]*Op. cit.*

[18]S. Baley, Versuche über die Lokalisation beim dichotischen Hören, *Ztsch. f. Psychol.*, 1914, 70, 347-372.

[19]*Op cit.*

[20]*Op. cit.*

[21]*Op. cit.*

[22]*Op. cit.*

[23]*Op. cit.*

[24]G. W. Stewart and O. Hovda, The Intensity Factor in Binaural Localization, *Psychol. Rev.*, 1918, 25, 242-251.

[25]Klemm, *op. cit.*, 3 Mitteil., *Arch. f. d. ges. Psychol.*, 1918, 38, 87.

at the two ears: a theory of phase-difference. Rayleigh[26] is the exponent of this theory. He holds that difference of phase is the principal condition of localization for the lower pitches. More and Fry[27] have a similar conclusion. Bowlker[28] reports multiple localizations as a function of phase-difference, Stewart[29] appeals to phase-difference as the principal factor in localization. Hartley[30] works out the mathematical relation between localization and phase-difference.

It would seem in general that psychologists have tended to turn to intensity and the physicists to phase-difference as the primary condition of localization. The fact that theories of phase-difference ordinarily take more account of the intensive factor than do the intensive theories of phase is due undoubtedly to the greater recency of the theories of phase.

(3) Klemm[31] has recently shown that, in case of a sound of short duration, the relative times of arrival of the sound at the two ears may condition localization. He finds that under the conditions for unitary localization priority of presentation is equivalent to the greater effectiveness secured by greater intensity, and thus seems to reduce both intensity and priority to some common factor of auditory effectiveness. In the writer's opinion it is possible that priority in phase may also ultimately be reduced to the same common denominator.

Binaural Localization of a Tone With Closed Tubes

In this first experiment the usual procedure with closed tubes was carried through in order to verify previous results with our observers and conditions and to obtain introspections with them. The following served as observers: Professor E. G. Boring (*B*), Dr. C. C. Pratt (*C*), Miss M. Bates (*A*), and the writer (*D*), all members of the laboratory group in experimental psychology. Dr. M. Yokoyama acted as experimenter for *D*.

Description of Apparatus and Method of Procedure

Two glass tubes of inside diam. of 5-16 in. were mounted so that each was perpendicular to the outside face of a 512 d. v. electric tuning fork, driven by a master fork of 256 d. v. encased in a padded box. These glass tubes were connected by rubber tubing of the same diam. to stethoscopic binaurals in a second room. In the latter room was a large table upon which a semicircumference of a radius of 2m. was marked off. Pasteboards with appropriate numbers were erected at intervals of 10° on the semicircumference to serve as localizing cues for the *O*, who with the binaurals in his ears was seated at the center of the circle. One of the rubber tubes in the first room was cut in two, and two glass tubes, each 90 cm. in length and one telescopically containing the other, were inserted in the breach, so that the length of this conductor could be reduced or increased by sliding the inner tube in or out of the larger. The larger tube was secured to a table upon

[26]Lord Rayleigh, On Our Perception of Sound Direction, *Phil. Mag.* (6 ser.), 1907, 13, 214-232.

[27]L. T. More and H. S. Fry, On the Appreciation of Phase of Sound Waves, *Phil. Mag.* (6 ser.), 1907, 13, 452-459.

[28]T. J. Bowlker, On the Factors Serving to Determine the Direction of Sound, *Phil. Mag.* (6 ser.), 1908, 15, 318-332.

[29]Stewart, The Function of Intensity and Phase in the Binaural Location of Pure Tones, *Phys. Rev.*, N. S., 1920, 15, 425-445.

[30]R. V. L. Hartley, The Function of Phase Difference in the Binaural Location of Pure Tones, *ibid.*, N. S., 1919, 13, 373-385.

[31]Klemm 4.

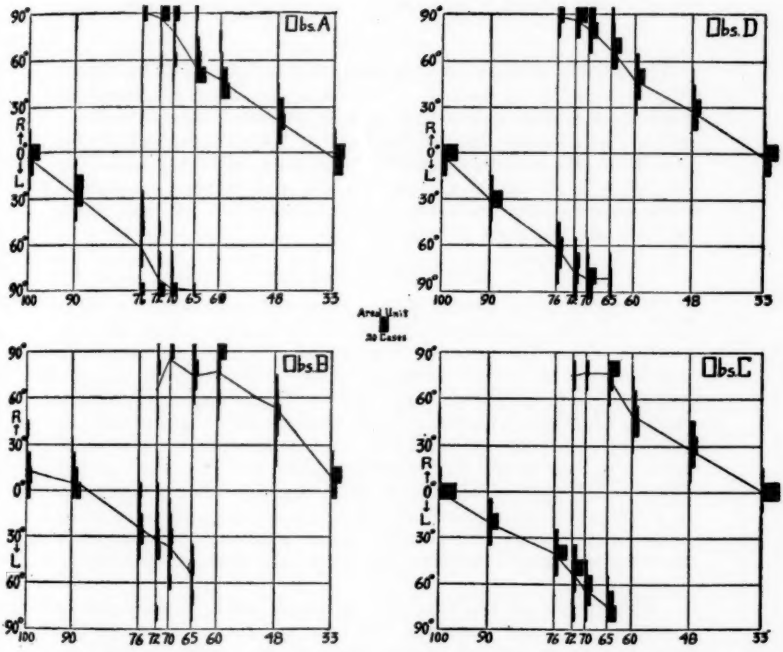


Fig. 1. Closed tube localization. Graphs of four *Os*, showing distribu tion of frequencies of localizations in degrees right and left of the median plane for the positions indicated on abscissa (cm.). Abscissa values are in terms of the scale of the apparatus (see text) and represent phase-differences of the tone at the two ears. The width of each chart, 67 cm., is the wave-length of the tone, 512 d. v., thus all phase-relations occur once in each chart. The areal unit representing the size of a block of 20 judgments for *A*, *C*, and *D*, should be read as 10 judgments for *B*. Double images occur at positions 76-65.

which a meter stick was fastened parallel with the tube. The open end of the inner tube served to indicate the position which had previously been determined by experimentation. Nine positions, selected because of the peculiar advantages they offered for studying our problem, were used.

With the *O* in position and the forks sounding, the apparatus was set for one of these positions. The *O* put the binaurals to his ears, localized the sound, removed the binaurals and wrote his judgment on a paper before him. The stimuli were presented in the order in which the numbers appear in the extension of the numerical value of π ('chance' order).

Results

The general schema for localizing was as follows. The *O* was told that he would be presented with an auditory stimulus the position of which he was to report in terms of degrees right or left of the median plane of his head. "Zero" represented any position in the median plane, "left 90" and "right 90" stood for positions in the aural axis at the left and right respectively, while intermediate positions were indicated by "left 10", "left 20", *etc.*, or "right 10", "right 20", *etc.*

Graphical Presentation

The quantitative results of the experiment are shown in Fig. 1. Here ordinates are drawn for the nine abscissa values experimented with. The abscissa is recorded in cm. of the scale attached to the telescopic tubes (*v. supra*). Ordinates are degrees right and left of the median plane. Upon each ordinate is erected a histogram showing the frequency of localizations which the *O* gave for the particular setting of the tubes. The areal scale is shown by the blocks at (70, R50) and (70, R20) in the chart for *B*, each of which represents a single case. Constancy in localization is indicated inversely by the amount of scatter of the histograms. The diagonal lines connect the averages of the distributions. The direction of the lines indicates the course of the image.

The graphs of the four *O*s are much alike. Localizations are median at the positions 100 and 33. From 100 to 76 the localizations are at the *O*'s left, from 72 to 65 double localizations occur, and from 60 to 33 the localizations are always at the right. The distance from one median localization to another is approximately 67 cm., which equals at 70° F. one wave-length of the tone used.[32] Double localizations take place one wave-length apart at points midway between the median localizations, a fact which we determined by further experimentation.[33]

A fair indication of the accuracy in localization is shown by the averages of the mean variations of the localizations for the four *O*s: *A*, 7.1°; *B*, 11.5°; *C*, 5.1°; and *D*, 5.6°.

[32] *Cf.* Bowlker, *op. cit.*; Wilson and Myers, *op. cit.*

[33] Bowlker's "two-image" points; *op. cit.*, 323.

B shows a decided tendency to refer the sound to his right. Localizations were frequently reported in the aural axis at his right but it is seldom indeed that they occur beyond 'left 50'.[24] An examination of *B*'s ears revealed that the left orifice is of slightly greater diameter than the right; hence it may be that the manner in which the stethoscopic binaurals fitted into these orifices in some way influenced localization. In testing his ears with an audiometer, no noticeable difference in hearing acuity was found.

Frontal and Lateral Localizations

The graphs show that the position of the image is most accurately determined when the localization is median and is increasingly less accurate as the localizations become more and more lateral. This inaccuracy according to the introspective evidence is due to two facts: failure to distinguish the position of the image both on account of its diffuseness and its vacillating nature, and on account of the failure of the *O*s to make accurate references to their localizing schemata. Since the schemata of our *O*s were visual, it is not surprising that peripheral localizations should have been inaccurate, since the visual field is poorly defined in this region.

In general it may be said that the lateral localizations are more difficult and less immediate than the frontal. The lateral image tends to be more diffuse and vague than the frontal, but is not necessarily so.

For example, *A* always made some such report as the following for the frontal image: "The frontal localization was immediate. I was fixating before me and heard the tone in the exact spot where I was focussing." "I was aware of kinaesthesis of my head giving the impression of left, so I said 'left 90.' This is much more difficult and the judgment is not immediate." Reports for all of *A*'s lateral localizations are similar.

B was less consistent, but always found the lateral localizations the more difficult. He reported once of a frontal localization: "The visual median image is small and intense, but easily localized;" and another time: "The visual median image is vague and of indefinite extent, but easily localized." The typical report for a lateral image describes it as "vague, diffuse and indefinite, and difficult to localize."

C always found the frontal and lateral images equally "clear" and "intense," although the lateral images involved "greater effort due to an inability readily to comprehend the visual schema." For example, *C* once reported as a summary of his experience: "At first the eyes are in a resting position, *i. e.*, looking in front of me, and the visual schema usually includes the area around zero. Now if the stimulus sets up an impression congruent with this kinaesthetic and visual set, the localization is naturally simple. If the impression is not congruent with this set, the visual schema must readjust itself and eye-movement sets in. If this movement is extensive there is awareness of kinaesthetic strains (meaning difficulty), though the actual localization is not difficult."

D, the most practised *O*, could distinguish no differences in the frontal and lateral images, except that the lateral tended to shift more readily under attention.

None of the lateral images compared was in the aural axis. When the image reaches the aural axis it is described by *A* as "more intense auditorily." For *B* the "visual context" was indefinite, "a vague misty blur."

[24] Hartley, *loc. cit.*, shows mathematically that the image of a 512 v. d. tone should move laterally as far as the aural axis of the observer.

C characterized the tonal image as "very intense," "elliptical," and "scattered." *D* said it was "scrambled, diffuse, piled up with an umbrage of surplus sound making it difficult to analyze."

That lateral localizations are more difficult to judge is Bowlker's view.[35] He states that within 20° on either side of the median plane localization is very accurate but that beyond 50° it may vary 10° or more.

Introspective Analysis

Localization of sound by means of conducting tubes is usually regarded as a simple process, in which the acoustic image appears to the *O* to be at his front, back or side, and in which he makes his report correspondingly. Psychologically the process of localizing is not simple.

For *A*, who kept her eyes open continually, the process of localizing the tone ran itself off in the following manner (analysis of 7 complete reports). (1) Very indefinitely localized perception of the tone began the process in all cases. (2) Then a movement of the eyes, a kinaesthetic pattern, indicating the general direction of the tone (6 cases). (3) Then imaged pressure or kinaesthesis on the head in the general direction of the tone (5 cases), which (4) becomes intense and definite (5 cases). (5) Finally the auditory perception is terminated by its being referred to the visual schema represented by the pattern for orientation laid out upon the table (6 cases).

B first perceives the tone indefinitely localized (analysis of 6 complete accounts). This phase is followed immediately by some portion of the schema for orientation laid out upon the table and by the eye-kinaesthesis involved in following the visual image of the tone along the schema (all cases). The image, as finally placed, was always a visual image with an auditory core, varying from time to time in hue, tint, clearness, and extent. The final judgment consists in the visualization of the number of the schema, which gives to the tonal image its local context (all cases).

C reports a non-focal visual schema of an arc before him extending on both sides of the median plane just preceding a fairly intensive auditory sensation (analysis of 6 reports). The latter is accompanied by the accrual of a more or less clear visual localizing context to the auditory core. The context consists of an area of the visual schema which gives the general direction of the tone (5 cases). This visual area rapidly narrows down to a clear spot (5 cases) with a definite position in the visual schema. Accurate determination of its position is given in visual directional lines that extend from the spot to the head (3 cases, implied in others). Judgment follows automatically in terms of this visual context. The more lateral localizations (all cases) are accompanied by slight strains of eye-muscles ('effort').

The fore-period for *D* contains a clear visual schema of the table pattern (analysis of 8 cases), with the most lateral portions added eye-kinaesthesis (7 cases). With the presentation of the stimulus the tone is experienced immediately as a spherical visual image (5 cases) which almost instantaneously assumes definiteness in form and position with reference to the visual schema present. Twice when the tone was weak the image seemed to appear only in auditory terms. Accompanying the image is eye-kinaesthesis of focussing on the image (5 cases). The judgment follows at once as the vocimotor image of the number at that particular point of the visual schema to which the image is attached (5 cases). Double images, one at each side, may be accompanied by intensive kinaesthetic imagery of the eyes as of sweeping the visual schema from side to side (2 cases).

[35] *Op. cit.*, 322.

Two general plans for localizing are thus shown in the introspections. The *O* may first obtain the tone and then apply its image to the visual schema as seen or imaged, or he may visualize the table pattern and see where the tonal image (auditory or visual) makes its appearance on the pattern. The first plan is more generally used, at least in the early stages of practice.

Rough approximations of direction occur usually in terms of eye-movement or eye-kinaesthesis with a given portion of the visual schema in the general direction of the tone standing out more prominently than the rest. Accuracy in direction is obtained when the visual or auditory image becomes attached to the table pattern or the visual schema.

The *O*s show a tendency to close the eyes while localizing, because the principal avenue of distraction is thus shut off, and also because the visual process (most *O*s tend to visualize the acoustic image) becomes clearer when the eyes are closed. *A*, who consistently keeps her eyes open, does not visualize the sound, but experiences it as an imaged pressure or kinaesthesis at a point on the head corresponding with the direction of the sound. Other *O*s at various times have had the same experience.

The visual context of the tone corresponds roughly with its auditory core in its sensory attributes. For example, a weak but clear tone is represented by a small clear visual image and a strong clear tone by a larger image also clear. An unclear tone, weak or strong, calls forth unclear and scrappy visual images.

Double Images at the Critical Phase-Difference of 180°

An interesting phenomenon is the disappearance of the acoustic image at one side and the subsequent appearance of a similar image at the other side, when the phase-difference of the tone at the ears is 180°. It has been suggested that these images are one and the same and that the image must therefore cross through the *O*'s head or behind it.[36]

Good conditions for studying this problem are afforded by this apparatus. The glass slide-tube was moved very slowly from the position 100 to 33 or reverse, changing the phase at the ears gradually so as to insure careful observation at the critical positions preceding, at, and following 180° difference of phase. When the *O* desired, a particular phase-difference was maintained and studied at length. The general results for the four *O*s follow.

A observed two auditory images, one at each side of her head, one less intense at first than the other. They differed in quality and were "two different images." Next "kinaesthesis swept rhythmically from one ear to the other and reversed" its direction. At one time she stated, with an image at the right, "I became conscious of something at my left but on looking there could find nothing. Then for a short time I was unable to tell on which side I was hearing the image, when suddenly it was at my left." Observing later under the suggestion that the image crosses the head, *A* stated that there was a rapid movement of the auditory image across the

[36] Stewart, Phase Relations in the Acoustic Shadow of a Rigid Sphere, *Phys. Rev.*, N. S., 1914, 4, 252; Theory of Binaural Beats, *ibid.*, 1917, 9, 518.

Rayleigh, On Our Perception of Sound Direction, *Phil. Mag.* (6 ser.), 1907, 13, 230.

head during which it "lacked intensity." The image "joined" the *other one.*

B at 180° difference of phase could not localize the image, or else a shifting kinaesthetic attention gave both left and right as correct localizations, or else the sound appeared always to escape to some other place than that which he was fixating. In trying to solve the dilemma he sought at times to place the sound inside his head, but "the localization was equivocal, since it was central and thus at no angle." At other times "the tone at one side would lose its auditory core and leave only the shell of the visual image. Then immediately I realized that the sound was at the other side of my head."

C in all cases imaged the sound at one side as a "large bright spot." There followed "rapid shifts of visual imagery" to the other side of the head "where a second image was also seen." Usually when the image reached the aural axis on one side, "another image appeared concomitantly at the other side. When the two images were equally clear the intensity was greatest. Finally the tone at the first side lost its intensity and its image gradually disappeared." *C* remarked every time that "the images were two distinct images and had nothing to do with each other."

D stated that when one auditory image reached the aural axis at one side it began to gain in intensity, while the sound, now clearly exterior, seemed to "surge into" the ear with the visual context dropping out. Then a slight kinaesthesis in the other ear caused him to look at that side, where he became aware of a "second auditory image weak at first but gradually waxing stronger until both images were of equal intensity. At this point both tones were exterior and surging into the ear. Besides they were diffuse, scatteringly intense, voluminous and unlocalizable except that one was all 'left' and the other all 'right.' Finally the first auditory image weakened and disappeared and only the second image remained, and the visual context began to accrue to it. Kinaesthesis of eye-movement and turning of the head with shifting visual imagery occurred during this entire procedure." At another time *D* stated that "one image originated at one ear, and the other passed away. The sound did not move from ear to ear. It simply accumulated in the images."

When the change of phase at the ears is accomplished *rapidly* an illusory movement of the sound image from one side to the other of the head at 180° phase-difference is evident. This illusory movement is given by the comparatively rapid changes in differences of intensity at the ears and not to a movement of the image itself. It appears to be similar in nature to stroboscopic visual movement.[37]

The evidence points to a rejection of the view that the image crosses through the head or behind it during the period preceding and following 180° difference of phase. It is clear that at 0° difference of phase the image is in the median plane, but no one has ever clearly imaged it in the median plane at 180°. Neither has any one clearly described how it crosses the head at this point. The illusion of movement with rapid changes of phase at this point is, however, decided. The fact that two distinct images may be observed for frequencies of 700 d. v. and above supports the conclusion that the image does not cross the head at 180°. When one of these images moves toward the median

[37]Peterson, *op. cit.*, 350.

plane, the second usually moves a short distance toward the aural axis, and, becoming weaker, finally disappears.[38]

Pseudo-median Localizations

B often reported localizations at 180° difference of phase as median. (See Fig. 1.) He explained, however, that the image was not median in the sense of being localized in the median plane, as is the case at 0° difference of phase, but that the sound was "central" or "at no angle" or "all around" or "equivocal." All the *O*s have experienced a "balancing of the sound," the having of the double sound of equal intensity at the two ears. *B*'s reporting this localization as median simply means that under an instruction for making a single localization he compromised the double localization as median. This is a common error which the writer has noticed for years. See p. 186.

Summary

1. The primary psychological factors which enter into the localization of a tone with closed ears, when the phase relations are altered, are (1) a visual, auditory, kinaesthetic, or tactual image of the tone or a combination of these, and (2) a visual or visualized schema of reference, in this case the 'arc-pattern.' The image varies in clearness, extent, and intensity depending upon its position, the intensity of the tonal core and the modality of the image. The 'arc-pattern' varies in clearness and extent with the different observers.

2. The fundamental psychological criteria for the formulation of judgments of position are: eye-movement or eye-kinaesthesis in the general direction of the sound, giving the rough meaning of 'right', 'left', or 'front', followed by adjustments of eye-movement or kinaesthesis in fixating the tonal image with reference to the visual arc-pattern.

3. Lateral localizations are more difficult than frontal because of (1) general inaccuracy in judgments of direction in indirect vision, (2) the diffuseness of the lateral tonal image, and (3) the apparent vacillating character of these images.

4. The tonal image as laterally perceived appears to be auditorily more intense, voluminous and diffuse than when frontally perceived. There is a tendency, allied to its diffuseness, for the visual context entirely to disappear in lateral localization.

5. When the phase-difference at the ears approaches 180°, the tonal image attains its most lateral position and gradually disappears. It is succeeded by a second image at the other side of the head when the difference in phase is again less than 180° and the phase at the latter side is leading. In the intermediate

[38] Hartley says that when "the image has reached a position near 90°, its direction remains unchanged, but in spite of equal intensity, it moves in toward the ear.... Beyond this point there is no corresponding position for an actual source and hence the curves tell us nothing as to where an image is to be expected;" *op. cit.*, 377.

critical position double images, one at each side of the head, may be observed. When phase alters rapidly through this critical position, the rapid succession of images may produce an illusion of movement of a single image through the head or behind it.

6. Observers may, under instructions to make single localizations, compromise a double lateral localization by reporting a median localization.

Binaural Localization of a Tonal Complex by Open Ears: Variation in Position of Dual Source of Sound

Wave-phase may be studied without the use of conductors to the ears. An account of the method and a description of the phenomena involved, as obtained by C. E. Seashore and the writer in 1918 at the State University of Iowa, are being published.[39]

The apparatus then used consisted of two telephone receivers connected in parallel from the same sound-source, with a two-meter stick suspended between them. The receivers were energized so as to produce a tone of 680 d. v. It was found that the *O*, by closing one ear and moving his head carefully from one source to the other with the side of his head parallel to the axis of the receivers, experienced a series of intensive maxima and mimima, which correspond exactly with the points of reenforcement and interference that obtain in the stationary wave set up between the two receivers.

When the *O* with both ears open (the aural axis parallel to the axis of the receivers) moves slowly from one receiver to the other, he localizes the tone in the median plane once for every one-half wave-length that he advances. These points of median localization correspond with the loop centers of the stationary wave.

Midway between the loop centers are the nodes. They are *critical* regions for localization. When the head is moved from a node toward a loop the image of the sound travels in the same direction as the head, but faster, toward the median plane from a position at the side from which the head is moving. The image reaches the median plane when the center of the head reaches a loop center. If the movement of the head is continued, the image then passes on out of the median plane in advance of the head until the next node is reached, when the image lies at the side toward which the head is advancing. Thus, if the head moves from left to right in passing from one node to the next, the image also moves from left to right, starting at the left side, swinging about the head, and ending at the right side. If now the head moves on from this second node to a third node, the image again moves from the left to the right as before.

We may summarize the situation for open-air localization as follows:

1. The space between the receivers may be thought of as divided into unit localization regions, each measuring one-half the wave-length of the tone and every one an exact counterpart of every other.

2. Median and balanced localizations occur alternately for each one-quarter wave-length of the tone used.

3. The movements of the image are experienced, not only in the line of the receivers, but practically anywhere within the range of audibility. These changes in other regions are not everywhere uniform, but at present no definite statement can be made as to their law.

[39] H. M. Halverson, *Univ. Iowa Studies in Psych.*, No. 8, 1921, monograph in press.

4. With low tones the image of the sound sweeps through a longer arc about the head than it does with high tones. At 512 d. v., *e. g.*, the image sweeps from 90° at the left of the median plane to 90° at the right, whereas at about 1500 d. v. the range of movement becomes so limited that the image is confined to the immediate vicinity of the median plane.

We set up the Iowa apparatus at Clark with the intention of repeating the experiment under more careful control and with psychophysical procedure. Instead of allowing the *O* to move his head, we had him bite on a biting-board and moved the two receivers suspended from the measuring-stick back and forth in the aural axis. A pointer on the biting-board indicated on the cm. scale that connected the receivers the position of the receivers relative to the *O*'s head. The apparatus was set at a definite position and the stimulus presented. When the *O* had localized the tone, the stimulus was shut off and the apparatus then set at another position. Stimuli were presented in chance order.

The tone was obtained, as at Iowa, by placing a small electric generator in the telephone circuit and driving it from a tonoscope used as a constant speed motor. In this way we obtained a frequency of 476 d. v. for which at 70° F. the wave-length is 72.4 cm. Two Edison cells were inserted into the field-magnet circuit of the generator to give the tone the desired intensity.

As soon as we started to work we discovered that the tone was not pure and that different upper partials could be distinguished in it. The second partial, 952 d. v., was very prominent, and in the vicinity of either receiver the more musical *Os* could distinguish still another partial. Attempts to secure purity of tone were unsuccessful, as is apt to be the case in work with telephone receivers.[40] It occurred to us, however, that it would be profitable to study the joint localization of the two more prominent partials, and see whether they operated independently, each in accordance with the law that we had verified at Iowa. Two of our more musical *Os* were able consistently to distinguish two partials, to identify them on a qualitative basis, and to make separate localizations of them. The other two *Os* were less ready at analysis and frequently localized only what appears to have been the more prominent partial. Since the intensity of a tone varies with its localization, that is to say, with the phase-relation of its stimulus, it was to be expected that in these cases sometimes one partial and sometimes the other would be predominant. Moreover, when the two partials lie at approximately the same angle, analysis is comparatively difficult, and the tendency to make a single localization is thus increased.

Ideally we should expect to find the course of each of the two images following the law already laid down. When the receivers are moved continuously with respect to the head, we should expect the two images to pass around the head through the median plane in the direction opposite to the motion of the receivers, and the image of the second partial to complete its cycle twice as often as the image of the first partial.

Except for Baley's dichotic experiments[41] the only instance of localizing with partials, as far as we know, is reported by Thompson[42] who caused the tones of two forks, 256 d. v. and 512 d. v., to be conducted by tubes to the ears so that in the first case the tonal complex was localized in the ears. Then, by causing the 512 d. v. tone to enter the ears in opposite phases, he obtained the higher tone localized at the back of the head while the lower remained localized in the ears.

---

[40] *Cf.*, *e. g.*, D. C. Miller, *Science of Musical Sounds*, 1916, 148 ff.

[41] S. Baley, *op. cit.*

[42] S. P. Thompson, Phenomena of Binaural Audition, II, *Phil. Mag.* (5 ser.), 1878, 6, 383.

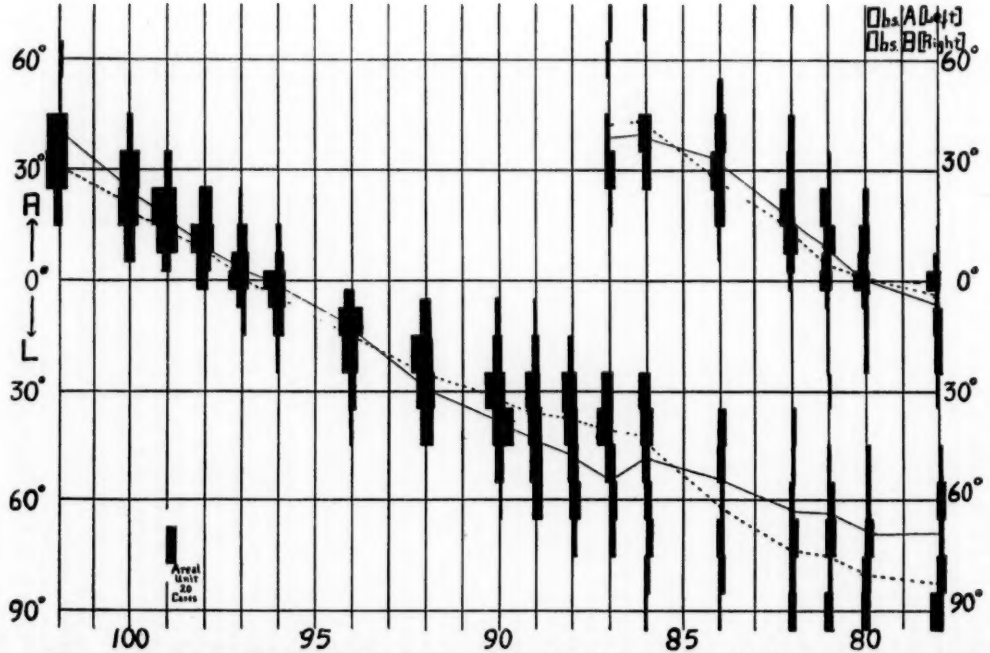


Fig. 2. Open ear localization. Graph showing distribution of frequencies of localizations in degrees right and left of the median plane for positions indicated on the abscissa (cm.). Abscissa values are in terms of the scale of the apparatus for localization with open ears (see text), and represent the position of the *O*'s head between the two sources of tone. Localizations by *A* (dotted line) and *B* (solid line) are indicated to the left and right of the ordinates respectively.

the two sources of tone. Localizations by *A* (dotted line) and *B* (solid line) are indicated to the left and right of the ordinates respectively.

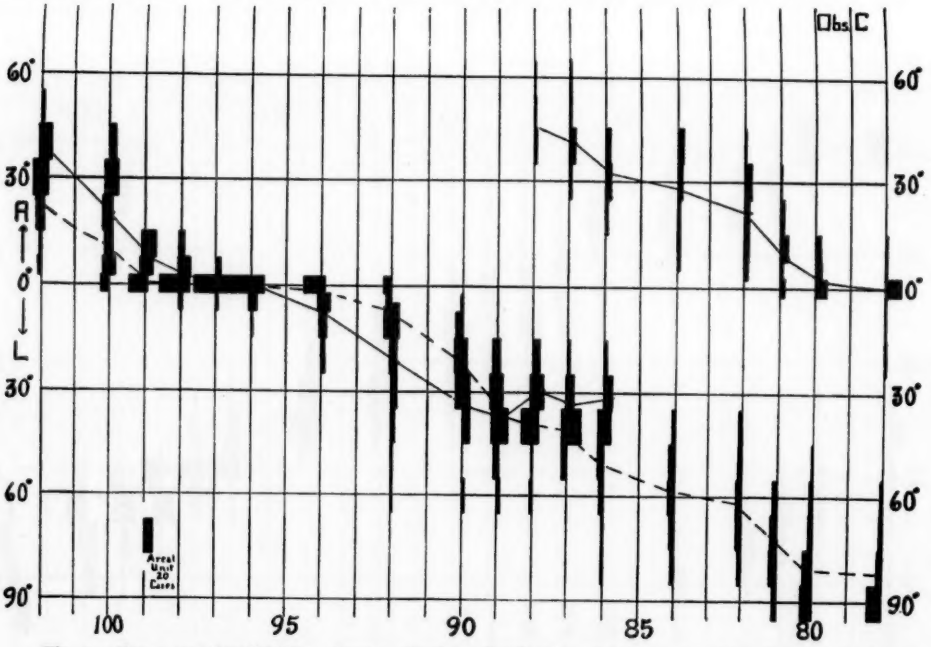


Fig. 3. Open ear localization. Graph showing distribution of frequencies of localizations in degrees right and left of the median plane for positions indicated on the abscissa (cm.). Abscissa values are in terms of the scale of the apparatus for localization with open ears (see text), and represent the position of the *O*'s head between the two sources of tone. Localizations by *C* for the 476 d. v. (dotted line) and the 952 d. v. (solid line) partials are indicated to the left and right of the ordinates respectively.

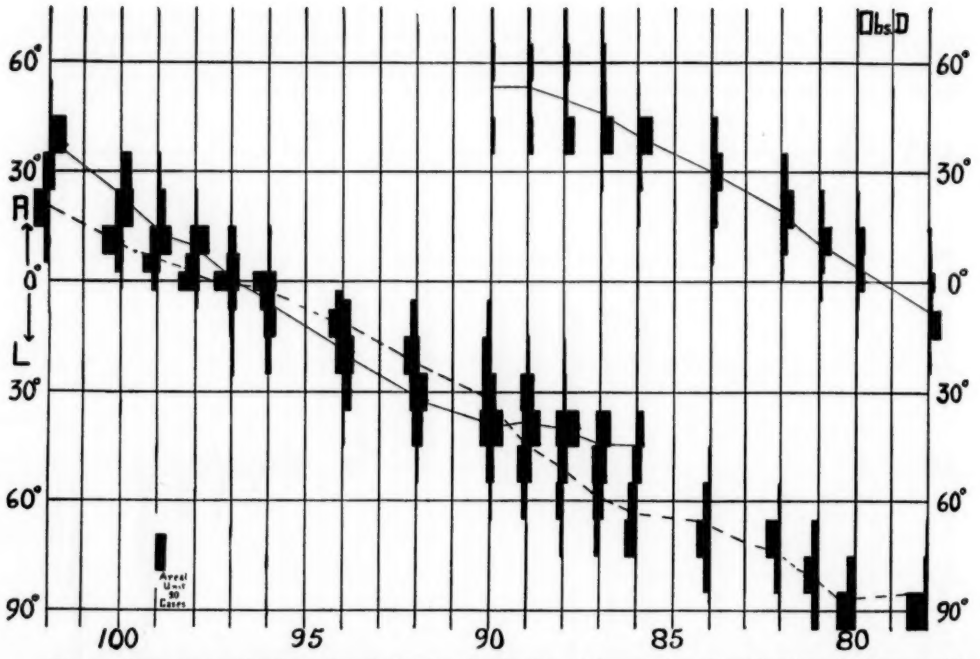


Fig. 4. Open ear localization. Graph showing distribution of frequencies of localizations in degrees right and left of the median plane for positions indicated on the abscissa (cm.). Abscissa values are in terms of the scale of the apparatus for localization with open ears (see text), and represent the position of the *O*'s head between the two sources of tone. Localizations by *D* for the 476 d. v. and the 952 d. v. partials are indicated to the left and right of the ordinates respectively.

Results

Graphical Presentation

The graphs of Figs. 2-4 were compiled from the results of four *O*s. Localizations were made in terms of degrees right or left of the median plane of the *O*. 'Zero' is any position in the median plane and 'left 90' and 'right 90' are in the aural axis at the left and right respectively. The graph of *O*s *A* and *B* shows the distribution of frequencies in localization in degrees at various positions (cm.) of the sound-sources with respect to the *O*'s head. *A*'s histograms lie on the left side of the vertical lines and *B*'s on the right.

The horizontal line [0°- 0°] represents the median plane of the *O*'s head. Right and left of the median plane are indicated respectively above and below this line. Numbers on the abscissa scale give the positions (cm.) at which the apparatus is brought at rest for purposes of localization.

Each of the ordinates, bearing a histogram, is a graphical representation of the *O*'s localizations at that position for the apparatus. The form of presentation is analogous to that for Fig. 1. Where two separated histograms appear on the same side of an ordinate (see positions 78 to 87), the situation is as follows: (a) either the *O* gave a double localization when the stimulus was presented (frequent occurrence), or (b) he gave one localization at one time and the other at another time under like conditions of observation. Occasionally double localizations were reported at 80 and 89, but these were so infrequent that the graphs fail to show them.

C and *D*, who were more musical *O*s than *B* at least, were better able to analyze the tonal complex into its primary components than were *A* and *B*. Their results are shown in Figs. 3 and 4, where the histogram on the left represents the frequency of localizations of the first partial and the histogram on the right the frequency of localizations of the second partial. It will be seen that both *O*s made double localizations for every presentation of the stimulus and in a great many cases triple localizations (see positions 78 to 88). For example, a typical observation of *C* or *D* at position 86 would be: Upper partial at 'right 40', same upper partial at 'left 40', lower partial at 'left 50'.

The graphs reveal in general that, for positions ranging from 102 to 98, localizations are made to the right of the median plane; from 96 to 88, to the left; and from about 88 to 78, both to the right and to the left. The images cross the median plane at two positions (97 and 80 approximately). The distance between these positions, 17 cm., is one-half wave-length of the upper partial. The course of the more prominent image (the upper partial) is very much the same for all *O*s from 102 to 88, and again from 87 on the right to 78.

Localizations near the median plane are less scattered than at the extreme lateral positions, a fact that corresponds with the assertion of the *O*s that lateral localizations are less definite.[43]

A and *B* never failed to localize the image of the upper partial. In the critical region (87-84) they generally reported two images of the upper partial, one at each side; and from 82 to 78 they often reported an image at the right which was recognized as this upper partial and another at the left which they could not always successfully identify; sometimes they were sure that it was the lower partial and at other times they were unable to distinguish it from the image of the upper partial at the right.

[43] *Cf.* Bowlker, *op. cit.*, 322.

scale of the apparatus. Localizations by *D* for the 476 d. v. and the 952 d. v. partials are indicated to the left and the two sources of tone. Localizations by *D* for the 476 d. v. and the 952 d. v. partials are indicated to the left and right of the ordinates respectively.

B's localizations are somewhat more to the right of those of the other *O*s (Fig. 2). Other *O*s often reported localizations as far left as 90° at position 78, but *B* seldom reported anything to the left of 70°. Rarely, however, he reported localizations to the right as extreme as 90°, a localization that no other *O* ever made. These reports occurred at positions 102, 89, and 88, but are too few to show in the graph.

The graph for *C* shows that images of both upper and lower partials are reported at each position of the apparatus. From position 102 to 89 both are reported regularly and are introspectively spoken of as "very adjacent" (*cf.* Fig. 3). At 102, 100, 92, and 90, where the images appear to be more definitely separated, the separateness is undoubtedly exaggerated. The images are reported as very close spatially; the anxiety of the *O* to set them apart as two distinct images seems to have caused him artificially to indicate a distance between them. For example, *C* often remarked that in localizing one image, he was also localizing the other as they were in the same lateral position, and yet he stated different localizations for them because of his total impression of their independence. In this region *A* and *B* (Fig. 2) failed utterly to distinguish two images; presumably they localized the two as one. Their failure to distinguish the images accounts for the greater variation of their localizations in this particular region. The images here are very close together and move in the same direction at about the same speed (as other observations show), and this similarity serves to make their perception only the more difficult.

From position 88 to 86 *C* generally reported three images, the upper partial at his right, the upper partial again at his left, and the lower partial at his left (Fig. 3). From 84 to 78 *C* reported always two images, the upper partial at his right and the lower partial at his left.

A feature of *C*'s observations is his tendency to localize toward the median plane. From 99 to 94 and from 80 to 78 the position of the images is predominantly medial. *C*'s introspective reports show that the images in passing from one side of the median plane to the other are localized high above his head, whereas the other *O*s localize the images at points in front of them. It appears thus that for the region considered a given angular change in the position of the image in the horizontal plane of the ears is observed as a somewhat greater arc than a similar angular change in the region of the zenith. *D*, who can at will place the image in front, behind, or overhead, stated that distances above 'seem less' than the same distances in front or behind. He also reported that the distance of the arc 60°-90° left or right is less when observed in the horizontal plane than when observed in the vertical plane of the ears.

D's graph (Fig. 4) is essentially the same as *C*'s. *D*, however, reported more triple localizations than *C* and showed no tendency to localize toward the median plane.

C's and *D*'s graphs show clearly how the images of both the upper and lower partials pass from the right of the median plane to the left. They also show that the image of the lower partial proceeded down to the region of 90, left. The formal experimentation stopped at this point, but incidental investigation showed that for positions beyond 78 this image gradually fades out while another similar to it appears at 90° right and moves toward the median plane. If the image is followed through and beyond the limit at the other end of the graph (102) the same movement is observed in reverse order. When the image of the upper partial has reached a point of about 45° left, it appears in most cases to be "fixed" there, that is to say, it does not move farther to the left as we should expect when the apparatus is moved. Remaining at this place it gets weaker and weaker and finally disappears (at the point where the image of the lower partial is most prominent). Occasionally *C* and *D* reported that the image of the upper partial moved with the image of the lower until it reached a point near 'left 90' when it disappears leaving only the lower partial audible on that side.

C and *D*, under instructions to report what takes place at the angle of 'balanced localization' while the apparatus is being moved, state that the diminution in the intensity and clearness of one image is accompanied by a simultaneous increase of the intensity and clearness of the other image. While the *O* is attending to the very clear image at the left, he finds, as this critical region of localization is approached, that a similar image, of weak intensity and not very clear at first, is claiming attention. Then as the apparatus is moved further along, a position (point of balanced localization) is reached where both images are of equal intensity and clearness; then a point where the second image surpasses the first in intensity and clearness; and finally a stage where the *O* perceives only the second image. The quantitative data (Table I) for these *O*s show clearly this phenomenon.

Bowlker, the first to report multiple images, calls the angle which this appearing image forms with the median plane of the head the "cross-over angle.[44]" This usage, it appears to the writer, is misleading, inasmuch as the image does not cross over from one side to the other. The angle may be called the "angle of balanced localization" and is thus referred to hereinafter by the writer.

From Hartley's calculations[45] for the location of the sound as a function of difference of phase, an angular displacement of the image of 90° right

TABLE I

Number of right and left localizations reported for image of upper partial at various positions indicated. Observers *C* and *D*.

Position of apparatus with respect to observer's head (cm.)	Observer *C*		Observer *D*	
	Left Image	Right Image	Left Image	Right Image
90	54	2	54	27
89	54	9	54	39
88	54	14	52	54
87	50	26	42	52
86	46	34	30	52
84	15	50	10	54

or left of the median plane for a tone of approximately 650 d. v. should occur for a phase-difference of 180°. For tones of somewhat higher frequency, two images should appear whenever the angular distance of an image is slightly less than 90° from the median plane.

Experimentally this conclusion is verified. The image of a tone of 680 d. v. does not attain an angular displacement of 90° before a similar image appears upon the opposite side of the *O*'s head. In fact at this frequency it is just barely possible to make out the two images simultaneously for a very limited portion of the total cycle of phase-differences.

In our work with a 930 d. v. tone double images appeared very distinctly when the *O*'s head was at the point of balanced localization (a quarter wave-length from the point of median localization). Here it was possible to move the head for a small distance either left or right without either of the images disappearing. Hartley's mathematical plot of the direction of the images of a 930 d. v. tone indicates that the angular distance separating the two images should be 102° when both are equally distant from the median plane (p. 381). In the same way he shows that three images, each separated from its neighbor by approximately 50°, should be observed for a tone of 1860 d. v. when one of the images is in the vicinity of the

[44] *Op. cit.*, 323.

[45] *Op. cit.*

TABLE II

Tabular analysis of description of auditory sensations and the corresponding visual images that occur in the simultaneous localization of two partials of a tonal complex under experimental conditions (see text) of localization with open ears. Numbers in parentheses are numbers of instances in which a positive report occurs in the protocols.

	Auditory Image		Visual Image	
Obs.	Upper partial	Lower partial	Upper partial	Lower partial
A	Clear (8), intense (8), thin (3), bright (2), high (10), rough (1), noisy (1), weak (1), unpleasant (2), definite position (5), lacks volume (1).	Clear (3), not clear (2), not definite or intense (2), low (6), varies in clearness (2), ear-kinaesthesis (3), round (1), full (1), voluminous (4).	Clear (2), definite (3), round (2), luminous (1), white spot with black center (1), black spot with yellow halo (7) shading off into grey (1).	Smooth (1), grey (1), and pear-shaped spot (1). Has yellow halo (1), indistinct (2).
B	High pitch (4) of stronger intensity at median plane than at lateral position (1).	Low pitch (4), ear-kinaesthesis accompanies tone (1), unstable (1).	Small (3), a spot (8), definite (5), grey (1), yellow (5), a fuzzy white ball (2) sometimes within a light mist (1).	Clear (2), not clear (2), faint grey spot (2), dark blue spot (4), black (1), just dark (1).
C	Piercing (4), intense (5), hard (6), rough (2), metallic (4), not pure (2), penetrating (7), complex (1), high pitch (18), of oboe or reed timbre (3).	Smooth (6), soft (5), intense (2), not intense (4), diffuse (7), low tone (18), like pure tone of tuning fork (6), more distant than upper partial (9).	Small (4), spherical (9), clear (11) and intense (5), hard (5), bright (4), luminous (3), extensive (1), nearer head than other image always.	Clear (3), not clear (4), small (1), large (3), smooth (1), round (1), oily (1), grey hue (3), dark (1), distant (9).
D	Sharp (9), intense (6), not intense (3), hard (1), definite and clear (11), sparkling (2), unpleasant, (2), impure (2), small (11), high pitch (10).	Soft (7), smooth (4), diffuse (2), extensive (2), thin (1), pure (1), clear (2), definite (1), intensive (2), humming (2), low tone (10), distant (11), fading (2).	Clear (9), intense (6), small (6), spherical (3), definite (8), depthy (2), bright (13), yellow spot (9) with grey halo (3). Like singing flame (1).	Soft (2), extensive (7), unclear (3), thin (3), bluish grey (7), very distant (3), larger than image of upper partial (4).

median plane. Bowlker[46] actually observed three such images with tones of 1675 d. v. and 2090 d. v. At 2310 d. v. he experienced difficulty although the images were present; and at 3050 d. v. he found localization practically impossible. We may therefore say of pure tones that, when the difference of phase at which the tone arrives at the ears is less than a half wave-length, two images are observable, and, when the difference of phase is decreased, a point is reached where it may be possible to observe three images.

Introspective Analysis

Limits of space[47] prevent an even partially complete presentation of the introspective data. The following accounts are of value as general indicators of the nature of the localizing process.

The description implies frequently the ability of the *Os* to distinguish between the two partials. Such distinctions were made by the *Os* partly in terms of the auditory core of the perception and partly in terms of the visual localizing context. Table II summarizes various attributes upon which these distinctions were based, and also throws light upon the qualitative nature and complexity of the perceptions involved.

Observer A begins always with (1) the focal auditory perception of the tone. (2) There follows a period of search, characterized by the kinaesthesis of eye-movement as the visual field is swept. There is a clear visual image of the schema of reference with some of the numbers representing localizations, upon it, and later the visualization of the tone upon the schema. Two tones may be thus simultaneously localized. So far, however, the localization is indefinite. (3) Then comes a rough stabilization of the image in terms of kinaesthesis within the head, giving roughly the direction of the tone; and (4) then the image is localized more accurately by placing it in visual terms upon the visual schema of reference, or rarely by placing the schema upon the tonal image.

Observer B gets (1) first an auditory perception of the tone, which is usually associated with a visualization of a portion of the arc of the schema of localization. (2) Immediately the tone is visualized clearly with definite qualitative characteristics. (3) Then comes a period of adjustment of the visualized tone to the visual schema of localization. Many numbers upon the schema may be clear at first, and drop off as the localization narrows down to a smaller region. The localization finally consists in the reduction of the visualization to the region of a single number. When there is a sudden shift in this localization accompanied by a change in the auditory quality of the core, *B* reports that he is aware of a second partial. (4) There is for *B* a characteristic process of searching, which may precede the localization just described, or may follow it as a verification. *B* sweeps the visual arc with his eyes (in imagery) until he detects the faint image of a tone, then scrutinizes this region to see if the visual image of the tone will become distinct and clear. If it does, he has still to "attach" the image to the schema by the process described above. (5) The process continues indefinitely by way of localizations and verifications, and ends with a verification. It is worth especial mention that, in cases of 'balanced local-

[46] *Op. cit.*, 324-326.

[47] *Cf.* footnote 1. The reader will have to accept the validity of the writer's generalizations or consult the bound manuscript in the Clark University Library.

zation,' *B* definitely reports a double localization (at the two sides) in the presence of a single auditory quality.

Observer C (1) begins always with an auditory perception of the tone, which is not focal, and a visual image of the arc of localization above the head, which is obscure. (2) Then the arc becomes distinct, the tone becomes focal, and a visual image representing the tone, is added to the auditory core. (3) Frequently this process of clarification is then reversed, and the tonal perception is replaced by the eye-kinaesthesis of searching, an occurrence which means that *C*, having localized the first partial, is now seeking the second. (4) The second partial is next localized in the same manner as the first. (5) Finally both partials are localized together. In this final stage, the two images may be focal simultaneously, or attention may shift repeatedly back and forth from the one to the other. The visualized arc at this stage is apt to become obscure, although *C* experiences no difficulty in making the verbal report of the two localizations.

Observer D (1) in all cases enters upon a localization with a visual image of the arc-path, extending from one side of the head to the other with its ends in the aural axis and making an angle in front about 50° above the horizontal plane. *D* does not, however, visualize the specific position of the schema of reference upon this arc-path. From this point on the process of localization varies with the position of the images.

(*a*) When the lower partial is near the aural axis and the upper partial near the median plane, then (2*a*) the upper partial is localized first, immediately and definitely. The localization is in verbal terms and is not mediated by visual imagery of the schema of reference. (3*a*) Then there is a vague awareness of the lower partial, which is at first located roughly by the eye-kinaesthesis, and (4*a*) is then definitely visualized and localized after the manner of the upper partial. (5*a*) In the final stage *D* verifies his tentative localizations by repeating the process just described.

(*b*) When the upper partial is in the position of 'balanced localization' and is therefore double, (2*b*) the two images of the upper partial appear immediately and simultaneously, and are of like appearance. *D* looks back and forth in imagery from one to the other. (3*b*) Then there is a period of search for the image of the lower partial, which ordinarily appears as faint, indistinct, and diffuse, and sometimes overlaps one of the images of the upper partial. (4*b*) Next the images of the upper partial are applied simultaneously to the visual schema, and (5*b*) then the image of the lower partial. (6*b*) The process concludes with a period of verification as in (5*a*) above.

(*c*) When the images of both partials are both near the median plane, (2*c*) the image of the upper partial is still first placed definitely within the visual schema, and (3*c*) the image of the lower partial, less distinct, is subsequently established. The remainder of the process of localization follows along as in the two cases already described, ending in a period of verification.

Summary

1. Two tones, separated by an octave, were reported by all four observers. Two observers were able to localize each tone of this pair independently of the other at every presentation of the stimulus. The other two observers were able to make this separate localization only at times. *See Figs.* 2, 3, 4.

2. The localizing context for all observers consisted usually in the visual image of a spot, approximately spherical, fixed in a spatial schema at a point from which the tone appeared to issue. This spot varied in clearness, hue, size, distance and other characteristics from time to time.

3. The distance from median localization to 'balanced localization' for the lower tone was approximately twice as great as the corresponding distance for the higher tone. This result is what would occur if the two tones follow independently the law of phase. *See Figs.* 2, 3, 4. For example, the distance that the receivers had to be moved to shift the image of the higher tone from the median plane to the position of 'balanced localization' was found experimentally to be 8.7 cm. This distance should correspond to a quarter of the wave-length of the second partial of 476 d. v., which at 70° F. is 9.0 cm.

4. The image of the second partial is ordinarily visualized as nearer to the observer than the image of the first partial. It is smaller and more distinctly outlined.

5. The image of the first partial shifts through a complete semicircle from 90° to the left of the median plane to 90° to the right of the median plane. The image of the second partial, however, shifts only through an arc of about 90° from 45° to the left to 45° to the right. For it the position of 'balanced localization' is thus at 45° left and right, not at 90°. *See Figs.* 3, 4. This result accords with other investigations which have indicated that with tones of higher pitch the angular range is limited and that the position of 'balanced localization' (the limiting position of movement of the image) is much less than 90° from the median plane.

6. The course of the image in passing through 180° difference of phase (point of 'balanced localization') is discontinuous. The image is either at one side of the observer or the other, or there are simultaneously images at both sides; there is no intermediate localization for the image.

The observers occasionally had the illusion of the movement of the image from one side to the other of the head when the receivers were moved at the critical position of 'balanced localization'. When, however, the stimulus was not moved and the observers were asked to describe the localization at this setting, they usually reported double images, *i. e.*, a reference of the tone to both sides simultaneously. It seems that the tone in this critical position is not ordinarily referred to a single point within the head, but that such a reference comes about only as a result of the tendency of observers to compromise a double localization under an instruction for making a single localization. See pp. 187, 195.

7. When the two images lie together in the region of the median plane it is extremely difficult to distinguish between them, and the observer tends to report a single localization of a tone of different timbre from either of the partials when they are localized separately.

8. The average consistency of localization was greater for the observers who habitually distinguished between the two partials. The average mean variation for these observers is 6.35°. The average mean variation for the other two observers is 10.3°.

Binaural Localization of a Tone by Open Ears: Variation of Relative Intensity of Two Sources of Sound

The fundamental problem of auditory localization is the essential condition of localization. Localization may be due to the difference of phase of the tones as they enter the two ears, to the relative intensities (binaural ratio) of the two tones as they enter the two ears, or to the difference in time at which impulses reach the ear drum (Klemm)[48]. The pendulum seems to be swinging toward difference in phase as the primary condition of localization, although the extent to which other factors contribute is not yet definitely made out. It is not impossible to see how phase-difference might be reducible to time-difference, and the last two theories harmonized. It was the purpose of these experiments to discover to what extent intensive differences co-operate in localization.

Our own experiments with the closed tubes favor a theory of phase-difference,[49] and it seems furthermore that phase-difference is also important in the localizations with open ears.[50] It is plain, however, that differences of intensity may exist at the two ears in open-ear localization, because of the existence of the standing wave with the two ears simultaneously at different regions of it. The actual relations of the intensities at the two ears depend on the relation of the width of the head to the wave-length of the tone. The image is, however, consistently referred to the side at which phase is leading, and this rule holds in many cases in spite of the fact that under it the image is referred toward the side where the intensity of the standing wave is weaker. In a sense, then, the localization may follow a rule of phase-difference even when the rule operates in opposition to the rule of intensive difference.

In incidental observation at Iowa and at Clark, the writer has frequently observed that a very great variation in the relative intensity of the two sources of sound in open-air localization (variation in electric current to the two receivers or in nearness of the receivers to the ears) does not seem to result

[48] See p. 180.

[49] See pp. 187-188.

[50] See pp. 198-200, and the writer's experiments at the University of Iowa, *op. cit.*, monograph in press.

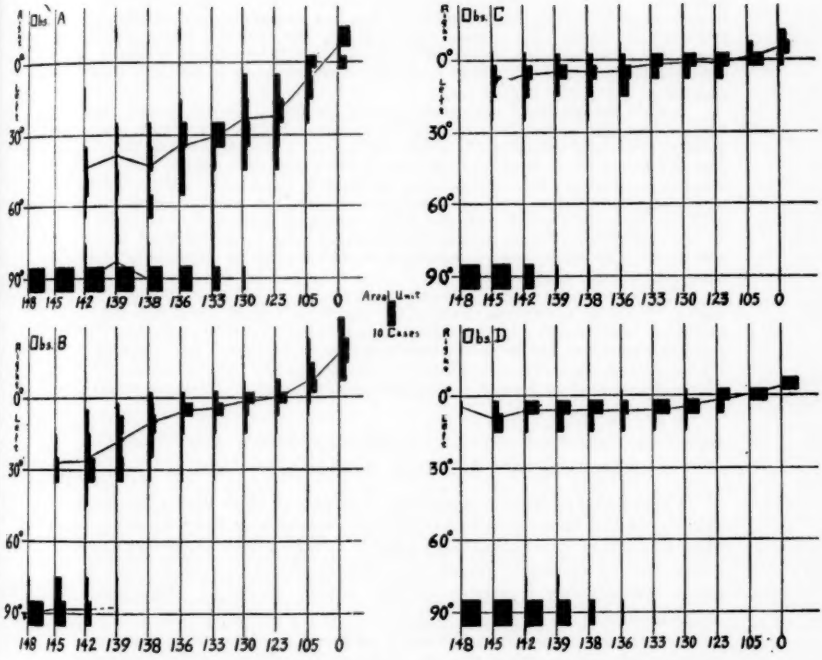


Fig. 5. Open ear localization. Graphs of four *Os*, showing distribution of frequencies of localizations in degrees right and left of the median plane for the positions indicated on the abscissa (mm.). Abscissa values are for the scale of the rheostat, and represent, electrically, successive values of the binaural intensive ratio: "0" indicates equal current to the two receivers on either side of the head, and "148" maximal intensity at the left with minimal intensity at the right. The medial and lateral images, distinguished by the observers, are shown separately on each graph.

in a movement of the tonal image as long as the phase-relations of the two sources remain unaltered. In the following experiments he has sought to investigate this matter systematically.

The same apparatus was used as is described in the preceding section of this Study except that the intensity of the sound at the receivers was reduced by removing the two Edison cells from the field-magnet circuit of the generator. The receivers were brought in toward the *O*'s head until they were but one-half wave-length apart. Thus only a few cm. separated the ear from the receivers. By placing the receivers but one-half wave-length apart, the head becomes a prominent barrier to the passage of a tone of this frequency (476 d. v.). There is thus no standing wave to be taken into account.

A slide rheostat with mm. scale (central position, "mm."; each extreme, "152 mm.") was inserted into the circuit of the receivers with each receiver in parallel with one side of the resistance. A shift of the slide therefore decreased the current through one receiver and increased it through the other, resulting in corresponding alterations in the intensity of the sound. At the extreme position of the slide one receiver was short-circuited and the other, in parallel with the entire resistance, maximally energized. In the experiments herein recorded the extreme ends of the rheostat were not resorted to since the receiver ceased to be effective as stimulus before short-circuiting was reached.

The *O* was to localize the sound, using the plan of the previous experiment. The slide was moved to one of the positions of the rheostat indicated in Fig. 5 (mm. scale) and the stimulus presented. After the localization had been reported, the slide was moved to another position and the same procedure followed. The stimuli were presented in the order in which the digits appear in the extension of the numerical value of π ('chance' order).

Series I: *Discrete Variation of Binaural Ratio with Tones in Phase and with Left Tone Always More Intense*

The receivers were connected in phase so that when the intensity at the two sides was equal, the sound was then localized approximately in the median plane. (When the receivers are connected in opposite phase, the localization is at once lateral even though the intensity of the sound is equal, 'balanced localization'.)

Positions on the rheostat were selected so that, with the exception of one position (0 mm.) where the electrical intensities are equal, the intensity of the left sound should always be greater than the right. Under these conditions we should expect localization to favor the left side throughout.

The results for each *O* under these conditions are plotted (Fig. 5) from the results of twenty-five observations at each position. The abscissa values on each graph are the millimetric positions on the rheostat at which the slide was set when the stimulus was presented and the ordinate values are degrees of localization right and left of the median plane. The positions investigated are spaced equally along the abscissa, although the intervals between them represent very different amounts of electrical change. These positions were chosen to give a large

number of points at regions critical for change in localization. The significance of the histograms erected upon the ordinates is the same as for the other charts discussed above.

The graphs are similar. The *Os* agree essentially in changes in localization caused by changes in the relative intensity of the sounds at the receivers. All tend to localize the sound toward the side of the greater intensity although they differ somewhat in the amount of this tendency. With the relative intensity at the ears equal (0 mm.) the localization is for all *Os* a trifle to the right of the median plane; the apparatus was apparently not precisely adjusted for median localization. At 105 mm. on the rheostat the localizations have taken a slight turn toward the left,—not a great difference, however, since the slide of the rheostat is moved 105 mm. out of the total possible distance of 152 mm. For each succeeding move of the slide the localizations show more and more a lateral tendency until a position is reached where, in addition to the image so far attended to, a second image, at first faint and elusive, makes its appearance at the *O*'s extreme left. From here on the first image wanes in clearness and intensity, though very slowly, while the second image waxes correspondingly stronger. [For *A* a point (139) is reached where regions of localizations for the two images overlap, although the images in each separate case remain distinct.] Subsequently (148) the first image disappears and the localization is of the second image alone. In a word, then, as the binaural ratio of intensities operates to favor one side more and more, the image moves toward that side until it reaches a point where it disappears. The point varies with the *Os* (8.0°- 43.3°), but in no case does the image approximate the aural axis of the observer. The illusion of a shift to the aural axis comes about because a second image is substituted in the aural axis for the first which disappears above the axis.

It is important to note the large differences required in the intensity-ratio (as shown by the millimetric distances on the rheostat) to effect small changes in localization. The slide of the rheostat was moved more than two-thirds its entire distance to the left (0-105 mm.: an electrical ratio greater than 5:1) to effect the first small change in localization, while no intensity-ratio was adequate to move this image in question laterally more than 22.1° (av.). The position of the second image was not affected by changes in the intensity-ratios. Some of the *Os* were more sensitive to the presence of this second image than others. The degree of sensitivity of the four *Os* to this image is indicated on the individual graphs by the number of cases in which the second image was reported.

Series II: *Discrete Variation of Binaural Ratio with Tones in Phase Throughout Complete Intensive Range*

This group of experiments differs from the first series in that the binaural ratio of the intensities was varied over the complete

intensive range from an extreme position with the right tone more intense to an extreme position with the left tone more intense. With each *O* 25 localizations were made at each of nine positions. These positions were selected in preliminary experiments so that they would yield data at the more critical regions of change and also so that they would offset the constant error of all *O*s to localize toward the right. The median position gave approximately median localization and the other positions localizations to the right and left respectively.

The results are shown in Table III. In each cell of this table the localization indicated at the right is of the frontal image, and at the left of the lateral image. As we have already seen in Series I, the frontal image never fuses with the lateral, and the two exist together in a certain critical region. For *A* and *B* the frontal image never becomes more lateral than 43° right or left of the median plane; for *C* and *D* it moves scarcely at all, never passing beyond 7° from the median plane. The average of the average mean variation of localization for all *O*s is 4.3°. The course of the frontal image is also shown in Fig. 6.

Series III: *Continuous Variation of Binaural Ratio with Tones in Phase throughout Complete Intensive Range*

The *O*s were now asked to note the changes in localization when, with both receivers sounding, the intensive ratio was varied continuously. The variation was accomplished by presenting one binaural stimulus first and then moving the slide of the rheostat. Two procedures were followed. (1) With the intensity of the two receivers approximately equal, the intensity of one was increased while the intensity of the other (necessarily) decreased. (2) With a difference of intensity between the two receivers, the intensity was varied so as to bring them to approximate equality of intensity. The movement of the slide was made by hand as uniformly as possible, and required, after practice, 35 sec.

The results accord with those obtained in the two preceding series of experiments, with one exception. There is reported by three *O*s a movement (which proved to be illusory) of the image moving from the median plane to the aural axis. When the intensities of the receivers are varied gradually from equality to a point where one completely overwhelms the other, the image moves from a position in the *O*'s median plane to a certain point, not far from the median plane (*cf.* Fig. 5), on the side which intensity favors. Here the image remains fixed, while a second image gradually becoming more intense appears at the same side in the aural axis accompanied by 'an appearance of movement' from the position of the first image to the position of the second.

That this movement does not involve the original image itself is shown by the fact that the latter may be observed in its fixed position after the 'movement' has occurred. The phenomenon is, as it were, a withdrawal of support from the first image in favor of the second. Upon reversing the intensive change the 'movement' recurs in the opposite direction.

The following analyses of reports indicate the nature of this 'movement' for the various *O*s. The numbers in parentheses represent the number of instances reported.

Observer A reported 17 times that in passing from the region of the median plane to the aural axis the sound image "splits up." The passage is not a "simple trip", but very complicated. The image in the region of the median plane first begins to fade. Then another image appears at the aural axis and soon becomes clear. While the image at the median plane is still clear, "something" appears to move down toward the image at the aural axis, although the movement is not so much a movement of the image as a "sweep or shift of the visual schema" with respect to the images. The 'movement' takes *A*'s attention from the first image; but after she has looked back she finds the first image still near the median plane (16). Only once was there no appearance at all of movement: one image faded out first and then the other appeared.

Observer B does not in the majority of cases get the 'movement'. In 24 cases the tonal image in the median plane faded out completely while the image in the aural axis was appearing. He reports, however, 11 instances of a phenomenon of 'movement' toward the median plane, when the intensive variation is toward equality of the binaural ratio. These cases are of two kinds. (1) With the image at the aural axis the "sound-mass stretches out," becoming larger and less definitely localized until it reaches the region 30° from the median plane. Here it "pours in," while going out at the aural axis. During this interval, the "tone" stretches from the aural axis to 30° "like a double paddle, with nuclei at these points and an attenuated strand of image between." Then the image at the aural axis fades out while the image at 30° gets "clearer, contracts, and moves slowly toward the median plane" (4 cases in all). (2) The tone starts at the aural axis, and then passes smoothly to 30° *without traversing the intermediate spaces*; then it proceeds slowly to the median plane. *B* was unable to explain how the 30° and the aural axis could thus appear to lie adjacent in his localizing space. He thought that his cue to the 'movement' involves "something more than visual imagery" (7 cases in all.)

Observer C. At first the image moves a little from the median plane (30). Then this image dies out and another appears at the aural axis (30). In the early trials the sound has the appearance of movement from one position to the other, "as though passing through an hour-glass or pipe-system" [*cf. B*'s "double paddle" above]; nevertheless the image does not move between these points. Later *C* sees the phenomenon as "a gray screen between 20° and the aural axis in which the image at 20° oozes out slowly while the image at the aural axis grows clearer" (4).

Observer D. The image remains in the median plane for a time and then moves slightly to one side (20). Then sometimes the image, with its accompanying tone, gets gradually weaker, while an unclear, diffuse "sound-mass" collects at the aural axis (13). Rarely in these cases it appears as if "something had slipped" from the frontal region down to the aural axis (2). At other times, however, a "cloud of sound" gathers at the first image on the side toward the aural axis, "stretching out vaguely" toward the axis (7). Subsequently this "cloud draws up" about the image at the aural axis (7). The total phenomenon gives the impression of movement without actual movement.

TABLE III

Average localization with open ears of frontal and lateral images with electrical variation of binaural intensive ratio. Phase approximately equal. The localization of he frontal image is shown at the right of each cell of the table; the lateral image at the left.

Rheostat Scale (mm.)		L148	L142	L136	L130	L105	R20	R80	R110	R130
Electrical intensive ratios		75:1	29:1	18:1	13:1	5.5:1	1:1.3	1:3.2	1:6.2	1:13
Observers	*A*	L90	L89 L43	L88 L35	L76 L28	L10	R8	R84 R22	R75 R30	R89 R36
	B	L82	L80 L32	L9	L7	R8	R70 R29	R81 R36	R80 R40	R90
	C	L90	L86 L6	L85 L6	L72 L5	0	R4	R5	R90 R7	R89 R7
	D	L90	L90 L7	L90 L5	L4	0	R4	R90 R5	R89 R6	R89 R7

TABLE IV

Average localization with open ears of frontal and lateral images with electrical variation of binaural intensive ratio. Left tone leading in phase. The localization of the frontal image is shown at the right of each cell of the table; the lateral image at the left.

Rheostat scale (mm.).		L148	L142	L136	L130	L105	R20	R80	R110	R140
Observers	*A*	L90	L89	L88 L61	L86 L50	L85 L44	L26	L9	R1	R89 R40
	B	L88 L37	L89 L36	L87 L31	L90 L29	L16	R20	R84 R27	R89 R36	R90
	C	L90	L90 L42	L90 L40	L85 L38	L32	L5	L1	R1	R90 R5
	D	L90	L90 L50	L90 L48	L90 L41	L40	L10	R90 0	R90 0	R90 0

Series IV: Discrete Variation of Binaural Ratio with Phase-Difference throughout Complete Intensive Range

In order to obtain some information concerning the relation of the effect of the variation of phase-difference to the effect of the variation of intensive ratio, we repeated the experiments of Series II with the left tone leading in a phase an amount necessary to bring the localization of the image to the region of 'left 30°-40°.' The change of phase was accomplished by shifting the receivers, still one-half wave-length apart, with respect to

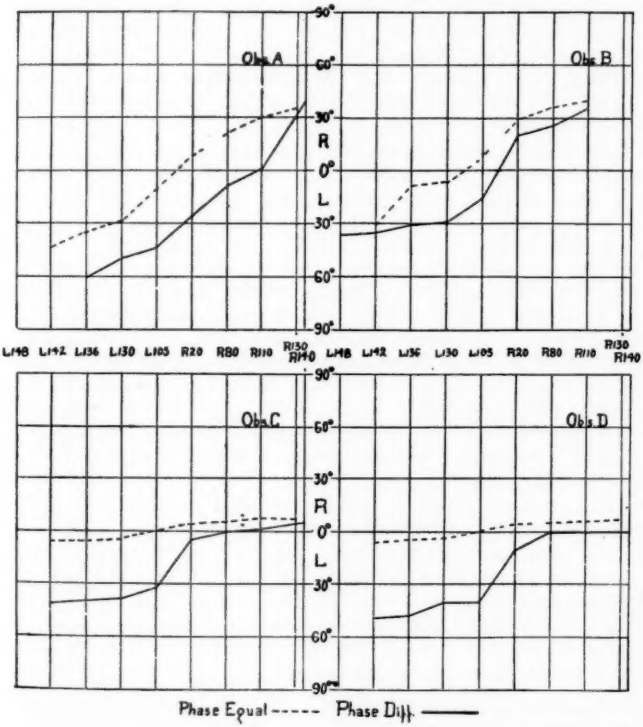


Fig. 6. Open ear localization. Graphs for four *Os*, showing localization (ordinate) as a function of binaural intensive ratio (abscissa). The abscissa values are settings of the rheostat controlling the relative intensity of the two sources of sound. See text and Tables III and IV. The dotted line is for the two tones approximately in phase, and the solid line for the left tone leading in phase.

the *O*'s head. This shift resulted also (presumably) in a slight intensification of the left source of sound, since this receiver was now nearer the head. The intensive difference is, however, to be thought of as having a small effect in comparison to the effect of the displacement of phase.

The observations are only slightly different from those of the three preceding series and do not alter the general conclusions. The frontal image shifts from 'left 47.5°' to 'right 20°,' the average of the four *O*s. It is never confused with the lateral image of the aural axis, but when it has attained its extreme lateral position all the *O*s report a peculiar unexplainable disturbance, as of sound widely distributed in the region separating this image from the image in the aural axis (see the introspective analyses: Series III, p. 205). The numerical results are shown in Table IV. The average of the mean variations for the localizations of all *O*s is only 3.2°.

Fig. 6 shows the effect that the introduction of a displacement of phase has upon the function that localization is of the intensive binaural ratio. The graphs of the figure show for each *O* the average localizations of the frontal image when there is no difference of phase (Series II: Table III) in comparison with the average localizations when the left tone leads in phase (Series IV: Table IV). It will be noted that in general the effect of introducing a phase-difference is simply to shift all localizations for a given binaural ratio toward the side of leading phase. In the cases of *C* and *D*, who show very little variation in localization when the phase-difference is zero, the variation is increased when the left tone leads in phase. This variation occurs on the side toward which there is greater latitude because of the change, *i. e.*, away from the shift due to the change in phase-relation.

The positions of the lateral image are not shown in Fig. 6, because this image always lies close to the aural axis.

The introspections show that where single images are reported the schema for localization for each *O* is that reported in the localization of a pure tone (pp. 187 ff.), and that the schema for double localizations is that described in the localizations of a tonal complex (pp. 198 ff.). In the present case, however, one of the images does not appear to move.

The following introspection by *B* sums up the situation nicely. He gives five other similar reports; and *A*, *C*, and *D* also report similarly although less fully.

"A very difficult localization. The tone at first was visualized indefinitely at my left between 90° and 30°. I do not mean that the visual images stretched from 90° to 30° for each image was small, subtending an arc not over 8°. The trouble was that I could not get the schema of reference to come into the visualization clearly. For a long time I sought (=eye-kinaesthesis of sweeping over the arc) for the place. *E. g.*, first I would

visualize the 90° position clearly. In doing this I could not see the tone! When I got the tone brought into the visual imagery, the imagery of the schema of reference would slip away. I would know that the tone was about 90° (*i. e.*, eye-kinaesthesis the same), but would not feel sure of the localization since the schema of reference was not visually clear. Then I would sweep through a small arc upward and the visual image of the tone would follow. About 30° this visual image would disappear and the visual image of the schema come in. Thus I got to know that the tone was between 90° and 30° without being able to say where. It also appeared as if 90° and 30° were only about 20° apart, since the sweep from one to the other was so small. I kept this chase up for a long time without getting the localization definite. Then I tried hard to see if I could get the visual image of the tone to come in while I had a clear image of 90° on the schema of reference. Soon I succeeded. The tone was distinct at 'left 90'.

While I fixated the tone here I became aware of another tone of the same auditory quality higher up on the schema of reference. I imaged it as in indirect vision. I turned my imaginal eye to it and saw it clearly, smaller than the other tone. At first it was unlocalized (indirect vision), then I got in rapid succession a number of visual localizations of it at about 40° to 30°. Finally the 30° reference became distinct. (This is an inadequate description of a long and very complicated consciousness, which is, however, a consciousness typical of the equivocal judgments that occur in these critical regions.)"

Supplementary Tests of the Rôle of the Binaural Ratio

We have found, when the binaural intensive ratio is altered, that the frontal image is limited in its range of movement, and that the lateral image, when it occurs, maintains an approximately fixed position. When both images are observed 'simultaneously', they may appear to be connected across the intervening space by "attenuated strands" (*B*) or "shooting streaks of gray" (*D*), or the connection may consist entirely in the fluctuation of attention between the two images (*A*, *C*) accompanied by eye-movement. If, with the stimulus sounding, one of these images appears when the other disappears, the *O* may experience a 'movement' from the one position to the other; it is not necessary, however, that this 'movement' should be anything more than a meaning applied to the successive occurrence of the two localizations. Except in this sense, no intensive difference is great enough to bring the frontal image near the aural axis. What happens ordinarily is that the frontal image becomes weaker as the lateral image becomes stronger, and that in this transition the auditory core, to which the visual image is the localizing context, seems to "detach itself slowly and subtly and to become attached" to the lateral image. Such a discrete shift of context may readily give the illusion of a continuous movement of the tone, especially if the localizations are not being made under an introspective *Aufgabe*.

There are some incidental tests which the writer has performed and which support this general conclusion concerning the effect of variation of the intensive ratio upon the localization of the tone. These tests are as follows.

(1) Series III in an abbreviated form was repeated for three *O*s, who, however, held the two telephone receivers close to the ears. The results, with the receivers thus placed, were exactly the same as those reported for Series III, (*q. v.*, p. 240 ff.).

(2) The intensity of the sound in one receiver was varied continuously by inserting into the circuit of one receiver an inductorium and varying the intensity inductively. The results of the localization of the image were again the same as in series III (3 *O*s).

(3) Three *O*s in localizing with closed tubes were asked to pinch one of the rubber tubes leading to the stethoscopic binaurals, after the appar-

atus had been set so that the image was localized in the median plane (phase equal). They all reported that the original image continued in the median plane until it disappeared. Meanwhile a second image gradually appeared near the aural axis. By careful manipulation of the tube these *Os* were able to find a condition (amount of pinching) under which both images were simultaneously observable.

(4) In open-ear localization with telephone receivers the image is always localized in the median plane when the *O*'s head occupies the position that corresponds with one of the maxima of the standing wave set up between the receivers (*q.v.*, p. 188). With a tone of about 700 d.v. and the *O*'s head in the position of one of the maxima, the receivers were moved one-quarter wave-length to one side. In this position the intensities of the two sources are approximately equal at the two ears, since the center of the head is now at a node of the standing wave; and, since the intensities are equal, the localization should be median. The *Os*, however, do not find a median localization, but experience the phenomenon that we have described as a 'balance' of intensities. The tone is heard on both sides at once and the image differs in extent, volume, timbre and intensity from the image of the median localization. If it be objected that this movement of the receivers does alter intensity slightly, since one ear is now nearer its receiver than is the other ear, it may be replied that the difference is inconsiderable in effecting localization, since, if the difference is made even greater by moving the next maximum to the center of the head, the *O* again gets a median localization.

Difference of Phase vs. the Binaural Ratio

We have already seen (p. 200 ff.) that the outstanding problem of binaural localization is the determination of the relative rôles of difference of phase and of difference of intensity in fixing the localization of the tonal image.

Rayleigh,[51] as we have seen, came to the conclusion in 1907 that the localization of low tones is a direct perception of difference of phase, whereas the localization of high tones (above 768 d. v.) is dependent upon intensive differences. Stewart[52] similarly holds that intensity is not to be considered as "an important factor in localization of pure tones" between 256 and 1024 d. v. Stewart found that when the intensity at one ear was diminished the image, originally in the median plane (equal intensity and no difference of phase), was usually displaced from the original position an amount depending upon the pitch of the tone and upon the particular *O*.

There is an early paper by Thompson[53] in 1878 which indicates the importance of phase-differences as effecting localization, although Thompson also noted the effect of intensity. He noted the change in localization of the tone when the connections to a telephone receiver at one ear were reversed while the connections to a receiver at the other ear remained unchanged.

In recent literature there is a tendency to consider intensive differences as mediating a rough localization and differences of phase a fine localization. Thus Bowlker[54] believes that "in the case of the higher notes—perhaps in the case of all notes—the zone or arc in which the sound-image appears is settled by the relative intensity at the two ears; the actual position of the images within this zone being produced by phase-difference at the ears." Klemm[55] found experimentally that, for sounds of short duration, the in-

[51] Rayleigh, *Phil. Mag.* (6 ser.), 1907, 13, 214-232.
[52] Stewart, *Phys. Rev.*, N. S., 1920, 15, 425-445.
[53] *Op. cit.*
[54] Bowlker, *op. cit.*, 327.
[55] Klemm, *Arch. f. d. ges. Psychol.*, 1918, 38, 88-91.

tensive differential threshold was significantly greater than the directional differential threshold for localization; that is to say, the image may move a distinctly noticeable distance in localization without the difference in intensity at the two ears becoming distinguishable.

The upholders of the binaural intensive ratio as the primary condition of localization cite the work of Matsumoto[46] in 1897. He used two telephone receivers, one at each side, simultaneously actuated by a 250 d. v. fork. When the relative intensities were altered the tone was variously localized about the head. These results seem to be inconsistent with the findings of the present paper. It must be remembered, however, that Matsumoto's tones must have been relatively impure, whereas ours were relatively pure in spite of the presence of the second partial in them. The addition of partials or noises to the fundamental alters conditions in an unpredictable manner. When phase is varied with an impure tone the various partials swing through different arcs to their respective angular limits (*cf.* p. 198 ff.); the relation of intensive change to the shift of the partials of a clang or the shift of different tones of different pitches is not known. Moreover, it may be true that Matsumoto did not always avoid varying the relative phase of his sources of sound, for he used an inductorium, the induction of which may have altered the electrical phase of the current. The greatest difference between the present experiment and Matsumoto's, however, lies in the lack of the introspective attitude on the part of Matsumoto's *O*'s. Our results have shown a certain amount of movement of the image about the median plane when the binaural ratio was altered, and they have shown that the tone (the 'second' image) 'reaches' the aural axis when the intensive difference has become great. Our introspections indicate, however, that the shift of the tone to the aural axis is discontinuous; it passes through a stage of equivocal localization where it is diffuse, scattered, or doubled. Matsumoto had no introspective check upon the nature of his *O*s' images; there is no certain way of telling whether the change for them was continuous or discrete.

In general then, it appears that a variation of the phase-relations of a tone at the two ears may lead to a continuous change in the localization of the tone, but that the variation of the intensive relations leads to a change that is continuous over a small region and discontinuous in its maximal change. Quantitative data, taken without introspective regard to the nature of the image of localization, may give the appearance of a continuous change of localization when the intensive relations are altered, because in the equivocal cases, where the discontinuity appears, an image that is double or scattered may be 'compromised' by being reported as localized at its geometrical center.

Conclusions

This study is a report upon the binaural localization of tones by four observers.

All these observers localized the tones in visual terms by placing a visual image that stands for the tone within a visual schema that represents the field of space. The analyses of the process of localization are given on pp. 186 ff., 198 ff.

The first part of the study shows that localization may be a function of the phase-relation of the tones at the two ears when the tone is conducted to the ears through closed tubes. The

[46] M. Matsumoto, Researches on Acoustic Space, *Yale Psychol. Lab. Studies*, 1897, 5, 1-75.

nature of the function is shown in Fig.. 1, and the results of this section are summarized on pp. 187-188.

The second part of the study exhibits localization as a function of phase-relation when the sources of sound are on either side of the head and the ears are open (no conducting tubes used). It appears in these series that the first and second partials of a tonal complex may be simultaneously localized and that each then follows the law of phase-difference independently of the other. The functions are shown in Figs. 2, 3, and 4, and the results are summarized on pp. 198-200.

The last part of this study shows the dependence of localization upon the intensive differences of the tones at the two ears (binaural ratio). It appears that with intensive variation the localization may move slightly, but usually remains in the region of the median plane of the head, except that with extreme intensive variation localization appears at either side of the head near the aural axis although it does not move there continuously. The shift of localization due to intensive change is thus discontinuous and not regular as it is for change of phase. It would seem that difference of phase is thus a more effective factor in determining localization than is the binaural ratio. These results are shown in Figs. 5 and 6 and in Tables III and IV, and the issue is discussed on pp. 209-211.

MACH'S "LECTURES ON PSYCHOPHYSICS"

By E. B. Titchener

In 1863 Ernst Mach, then *Privatdocent für medicinische Physik*, gave at Vienna a course of lectures on psychophysics.[1] We fortunately possess the reports of this course furnished by Mach to the *Oesterreichische Zeitschrift für Praktische Heilkunde* and there published in the same year. I believe that the lectures are practically unknown; and as they seem to me to be of more than merely historical interest I have made a brief summary of the contents of the reports,—a summary, be it remembered, of what is already part-summary and part-excerpt; the reader must expect nothing more than the barest outline. I have followed the divisions of the *Zeitschrift*, though I am not sure whether they represent separate lectures or available editorial space. Apart from the general headings, which I have ventured to supply, the rest of this paper is, then, a condensation of Mach's work.

The Science of Mind

§1. There is no special group of 'exact' sciences. Whether a science is or is not 'exact' depends solely on the stage of development to which it has attained. In fact, there is no reason why psychology (the science of the phenomena of the mental life) and psychophysics (the science of the interconnection of physiological and psychological phenomena) may not, if they follow the path they are already traveling, presently become in the fullest sense of the term 'exact' sciences.

[1] See my *Experimental Psychology*, II., ii., 1905, xlvi. The reports were reprinted in a little book, entitled *Vorträge über Psychophysik*, which I have never seen. For several years I sought it assiduously, but in vain; finally, I wrote to Mach himself about it. He replied that he knew of the existence of only one copy, which was in his own library; and with his usual kindliness—in such matters he was the soul of generosity—he offered, since I was seriously interested, to lend me the precious volume. I could not, of course, accept that risk; and as a reward of virtue I presently picked up the volume of the *Oester. Zts.* from which the present summary has been made (ix. Jahrgang, 146, 167, 202, 225, 242, 260, 277, 294, 316, 335, 352, 362). The volume, as it happens, contains also Mach's review of Helmholtz' *Tonempfindungen* (915, 930, 953).

What are today the 'exact' sciences? Mechanics, applied mechanics, physics in so far as it is applied mechanics, physiology in so far as it is applied physics, the science of man in so far as it is statistical,—all these disciplines rest upon exact laws. If no law can be found, that is either because there is no law to find, or because the law is too complex for immediate detection.

§2. Physical experiments made, in accordance with the Baconian rule of varying the magnitudes involved, with the view of establishing a natural law, always yield discrepant results. Extraneous influences are at work, influences which we sum up under the name of 'chance'. But chance itself is subject to law. Quetelet has shown that, as regards man, not only height, increase of population, etc., but also crimes, suicides, marriages, wrongly addressed envelopes, etc., are subject to numerical law; that is to say, the moral and intellectual elements of our social life, the psychological processes, are no less uniform than the rest.

We ourselves shall be mainly concerned with psychophysics, whose results (as Wundt has recently shown) are of practical importance for medicine. Meantime we must briefly consider Herbart's psychology, as the first attempt to deal with psychological phenomena from the mathematical point of view. The attempt is by no means ridiculous, since ideas, if not measurable magnitudes, at any rate vary in intensity and by that property admit of mathematical treatment.

Herbart's Psychology

§3. Herbart's psychology deals with artificially simplified phenomena, just as mechanics does. Ideas once aroused are, for him, indestructible; if driven from consciousness by other ideas, they persist as tendencies to ideation (cf. the law of inertia of matter). Disparate ideas are compatible; similar ideas are more or less antagonistic. Inhibition is mutual, and therefore minimal; its amount in the given case is inversely proportional to the intensity of the ideas concerned (Wundt has tried to show experimentally that the weaker of two ideas, even at an extreme difference of intensity, is never wholly suppressed); and the clearness of the ideational fragments remaining after it has done its work varies roughly as the square of the intensity; *i. e.*, an idea twice as strong as another remains about four times as clear as that other, and so attracts the attention.

A number of concurrent ideas may establish an equilibrium, with complete suppression of weaker ideas; mutual inhibition then operates as if these weaker ideas did not exist. Hence we find but relatively few ideas present in consciousness at any given time, and hence we are not burdened and restricted by the ideas we have forgotten. Equilibrium is not achieved in a

moment; the nearer it is approached, the slower is the advance towards it; indeed, it is never absolutely complete. In this way we account for the motility and variability of our internal states.

Ideas which concur in consciousness form permanent connections, and form them the more completely the freer the concurrent ideas are from inhibition. Here is the explanation of association and mediate reproduction. A series of ideas, whose earlier terms are increasingly inhibited as the series proceeds, allows only of progressively weaker connections between these earlier and the later terms; if the whole series has been inhibited, and presently a member returns to consciousness, the terms are reproduced in their original order. Herbart thus explains not only our remembrance of poetry but also the building up of our spatial perceptions.

Herbart's mathematical results come so near to the facts of experience that we must believe him to be on the right path. The good observer can, so to say, actually feel within him the struggle and mutual suppression of his ideas. But we turn now to experiments on sensation. If we find that sensations are measurable, we remove one of the principal objections to Herbart's psychology.

The Methods of Psychophysics

§4. The problem of psychophysics is to determine exact relations between stimulus and sensation by the way of observation and experiment. We have to measure both stimulus and sensation. The measurement of stimulus is simple; but sensation can be measured only by recourse to stimulus. It seems that we are involved in a circle.

Fechner, however, has shown us how to overcome this difficulty. We measure sensation by aid of the just noticeable difference of stimulus, which corresponds always with an identical increment of sensation, and thus furnishes us with a sensation-unit. We must, it is true, employ different sensation-units for the different classes of sensation, and cannot reduce them to such common terms as physics has found in mass, space and time; but there is ground for hope that a reduction may presently be effected.

The special methods of psychophysics seek to determine points on the path of a curve, *i. e.*, to ascertain for a selected number of stimulus-values those increments of stimulus which condition determinate increases of sensation. They are as follows.

Method of Just Noticeable Differences.—"For a series of stimulus-values x_1, x_2, x_3,.....we determine the just noticeable differences Δx_1, Δx_2,.....which make $x_1+\Delta x_1$ just distinguish-

able from x_1, $x_2+\Delta x_2$ just distinguishable from x_2, and so on, and which therefore in every case correspond with one and the same increment of sensation Δy. If it turns out, *e. g.*, that Δx_2 is twice as large as Δx_1, it necessarily follows that for the stimulus x_2 the rise $\Delta y/\Delta x$ is only half as large as for x_1. If we should find in general that the 2, 3, 4,.....*n*-fold stimulus has the 2, 3, 4,.....*n*-fold just noticeable difference, we could infer that $\Delta y/\Delta x = a/x$, *i. e.*, the increase would be inversely proportional to the stimulus-magnitude x. Mathematics would then draw the further inference that $y = a$ (log x/b), where a and b are constant magnitudes."

Method of Average Errors.—"We take a constant stimulus x and try again and again to make a variable stimulus x_1 equal to it; we determine the error of every trial, and from a large number of these errors calculate the average error. In comparing our sensations we are thus subject to error for the reason that a series of accidental circumstances influences our judgment. For example, we take the two sensations y and $y+\Delta y$ and therefore also the two stimuli x and $x+\Delta x$ for equal, and so make the error Δy in estimating sensation and the error Δx in estimating stimulus. The error Δy, by the laws of probability, cannot exceed a certain magnitude. The average error will also, in a long series of observations, keep within certain limits of magnitude. It is, however, clear that one and the same error Δy will have corresponding with it a larger error Δx, the more slowly Δy varies with Δx, *i. e.*, the weaker the rise of the sensation-curve. Hence the average error in our estimation of the stimulus x is inversely proportional to the rise of the sensation-curve for the stimulus x."

Method of Right and Wrong Stimuli.—This method "is a modification (or rather, inversion) of the method of average errors."

The Facts of Psychophysics

§5. We pass from the methods to the facts of psychophysics.

I. *Weber's Law*, so named by Fechner, declares that the just noticeable difference is proportional to the magnitude of stimulus. Fechner in particular has shown that, within certain limits, the law holds in the most various departments of sensation.

(1) *Intensity of Sensations of Light.*—Here belong Fechner's experiments with cloudlike figures painted on white paper; Volkmann's experiments with shadows; Masson's observations of grey-ringed white discs. All experiments show that not absolute but relative differences are important for us. A stim-

ulus must increase by a determinate aliquot part if the increase is to be remarked; and the same stimulus-difference is noticeable or unnoticeable according to the magnitude of the stimulus.

The law does not hold for very high and very low degrees of illumination. Helmholtz' experiments with rotating discs seem to show that it holds, in any case, only approximately. Fechner contends, however, that the external stimulus must always be increased, for purposes of calculation, by the amount of the intrinsic retinal light (determined by himself and Volkmann as equal to the light intensity of a black velvet surface illuminated by a stearin candle at a distance of 9 ft.). The results of observation are in this way brought into much closer agreement with the law.

(2) *Intensity of Sound.*—The experiments of Fechner and Volkmann confirm the law.

(3) Our sensations of *tonal pitch* and *interval* follow the law (Herbart, Drobisch); our sensitivity to *color* does not.

(4) Weber found the law to hold for our sensations of *resting and lifted weights.* Fechner's numerous and exact experiments with lifted weights give a good agreement with the law if the weight of the lifting arm is taken into account.

(5) Fechner believes, as against Weber, that we may have an uniform sensation of *warmth* or *cold.* It seems that the law applies to temperatures which differ but little from the mean (unsensed) temperature; it certainly does not apply to those that depart widely from the mean.

§6. (6) According to preliminary experiments of Fechner's the law holds for *mixed* sensations (colors).

(7) It holds also (Fechner, Volkmann, Appel) for *distances* and *lengths,* which are probably the resultants of a complicated psychological process. In the case of distance, *e. g.,* the facts of observation are satisfied if we regard the average error as made up of a constant error, identical for all distances and due to the division of the retina into a finite number of sensitive elements, and a variable error, proportional to the distance.

(8) Czermak's work on the sensation of *time* is preliminary only. Mach's experiments with pendulums (1860) prove that the law applies to this sensation.

II. A second fundamental fact is the *Law of the Limen.* Psychophysics distinguishes stimulus and differential limens, intensive and extensive limens.

III. Fechner's *Parallel Law* (that reduction of sensitivity has, at least in many cases, the same effect as reduction of stimulus) needs further investigation. It seems to hold for lifted weights, but not for sensations of light.

Fechner's Interpretations

§7. Weber's Law may be formulated as $\Delta y/\Delta x = a/x$, where a is a constant. By purely mathematical reasoning we derive from this expression Fechner's metric formula $y = a \log (x/b)$, where b is a second constant. Since $x = b$ means $y = 0$, x must be $> b$ if y is to attain to a finite positive value; b then denotes the liminal value of x. The Law of the Limen would therefore be contained in Weber's Law if Weber's Law itself were unconditionally valid. As things are, the laws stand side by side, empirical and not contradictory.

The metric formula may be made definite if we take as unity the sensation which corresponds with some determinate stimulus-magnitude, and define a accordingly.

This constant a is a measure of absolute sensitivity; it is by no means to be confused with the liminal value b. At present, it is true, we cannot apply a measure of absolute sensitivity, since we cannot compare the magnitudes of the sensations which, in different states of sensitivity, correspond with the same stimuli.

Fechner's further arguments regarding the 'aggregate' sensation and its summation from single sensations are noteworthy and interesting; but neither his psychology nor his mathematics is free from objection.

§8. Fechner extends his metric formula, first, from differences of sensation to sensations of difference, which he thinks enter into the aggregate sensation, under certain conditions, as sensations of contrast; and secondly, in the instance of tones, from a single dimension—pitch or intensity—to the two dimensions taken together. Again, his arguments are open to objection. Mach (1862) applies the formula to our sensitivity for the change of position of straight lines; theory and experiment are here in good agreement.

Sight and Hearing

Our path now takes a new turn. Natural science begins always with problems that force themselves on the attention; involuntary enquiry precedes voluntary, as reflex precedes voluntary movement. Laplace's planetary theory arose from the insistent demand of the facts themselves. And, as in astronomy, so it is in psychophysics. Light-stimulus and sound-stimulus are both alike oscillatory in nature. Why, then, does tonal pitch obey Weber's Law, and color refuse to obey? Why does the tonal series show the periodicity of the octave, and the color-series lack periodicity? Why are tonal compounds analysable, and color-mixtures unanalysable?

These questions can, in large measure, be answered. We come presently to the special investigations (Young, Fechner,

Helmholtz). Here we must refer briefly to the laws of oscillatory movement under the influence of variable forces (Seebeck, Mach), laws which explain, among other things, the phenomenon of sympathetic vibration. Our interest lies in the theory of hearing; and we find in the tuned series of Corti's fibres an apparatus which, in terms of these laws, enables us to account for tonal analysis. The beats which arise when two near-lying tonal stimuli act on the same fibre, and which cause a disagreeable sensation, solve (or at any rate simplify) the problem of consonance and dissonance.

§9. Musical tones consist of fundamental and overtones, which latter determine timbre. If two or more musical tones are sounded together, we hear a dissonance or a consonance according as fundamentals, overtones or combinational tones do or do not engender beats. If we sound a tone with its first overtone only, and gradually raise the pitch of the tone to its octave, we experience a 'feeling of recurrence', of the periodicity of the octave-interval. Other intervals show analogous phenomena.

The theory of Corti's fibres was proposed, almost simultaneously, by Fechner and Helmholtz. How shall we test it? (*a*) Fechner suggested that, if single fibres are incapable of vibration, the ear will lack the sensations of the corresponding tones. Politzer confirms this suggestion. (*b*) Unilateral affection of fibres should bring it about that the two ears hear the same tone differently. Cases have been observed. (*c*) Any lessening of the capacity of the fibres for vibration should decrease our sensitivity to tonal differences. (*d*) Since the fibres are limited in number (about 3,000), our estimate of very small differences of pitch will perhaps prove to be subject to a special constant error (cf. what has been said of the estimation of spatial distances).

We have seen how the sensations of color differ from those of tone. We add (*a*) that the mixture of two or more spectral colors gives rise in all instances to a color which can be represented by white and an intermixture of a single spectral color; and (*b*) that any mixed color whatever can be produced by the mixture of three properly chosen spectral colors. Brewster thought, accordingly, that there are only three objective colors; but Helmholtz has proved the continuity of the spectral series. Helmholtz and Fechner (who, again, published almost simultaneously) therefore suggest, following Young, that there are three fundamental subjective colors, red, green and violet. But Helmholtz believes (with Young) that each one of these colors has its own nerve-fibre, while Fechner assumes three different kinds of process within the single nerve-fibre. The point at issue will be decided the one way or the other according as the retina of

the partially color-blind (who lack a green-sensation) turns out to be defective in anatomical or in chemical elements. Or we might examine the color-blind periphery of the normal retina.

Since every objectively simple color is subjectively compound, no one has ever seen a simple color. If we look at a light for some time through a red glass, and suddenly exchange the red for a green, we see a green of quite unusual purity.

In summary: "the eye complicates the objectively simple, the ear analyses the objectively complex. On the other hand there is also a real resemblance. We might say that the eye is a mosaic of light-ears, with the number of Corti's fibres reduced in every ear to three.—These differences seem to account for at least the greater part of the differences in the behavior of our sensations of light and sound."

§10. Helmholtz' extension of the doctrine of specific energies, which follows on his theories of color and tone, is to be regarded with caution. Helmholtz thinks that not only sensations but also qualities of sensation have their special nerves; since, however, we find in the nerves only electrical currents, these nerves must be like telegraph wires, and simply transmit certain signals that correspond with the sensations. We know, however, too little of nervous processes. What we know comes, it is true, by way of electrical experiments; but these experiments, delicate as they are, are also too crude; effects that seem to be physiologically the same need by no means be physically identical.

The Problem of Perception

We have already objected to Fechner's view of the aggregate sensation as a mere sum of simple sensations. Wundt has recently shown (1862) that between sensation and perception there lies in fact a series of psychological processes, which he calls 'unconscious inference.' Helmholtz had previously come to practically the same conclusion. Wundt, *e. g.*, has proved the dependence of the perception of distance upon accommodation and convergence; he has proved further that there are no 'identical' retinal points. All Wundt's experiments on visual space-perception indicate that the sensations of the two retinas come separately to consciousness, and only there are connected, by a sort of inference, to a stereoscopic perception; and what holds of stereoscopic vision holds also of mirroring and lustre.

What now is the function of these unconscious inferences? Kant regarded space and time as forms of perception given a priori: but why then should some sensations (sight, touch) fall into the spatial schema, and others not? Weber makes the brain repeat the peripheral arrangement of nerve-endings: but here is not even an explanation of tonal analysis, to say nothing of

space-perception; and even if the theory be taken to account for our separate hearing of partial tones, it would still be inadequate to space, since the two modes of perception are not identical. Herbart explains space-perception as a case of serial reproduction: but on this hypothesis, again, all the senses ought to be spatially competent.

§11. Lotze, in his doctrine of local signs (which may be either a system of concomitant sensations or a system of movements, or rather of impulses to movement), attempts a combination of the anatomical and the psychological views. Wundt's theory is an extension of Lotze's. The part which Wundt ascribes to muscular feeling (against Schiff) is rendered highly probable by the results of his experiments on the estimation of distance. His theory takes account not only of visual space-perception but also of the tactual space-perception of the blind and the seeing. It is supported by observations of pathological phenomena (paralysis of the ocular muscles). Wundt has further observed that the 'sensory circle' very quickly changes under the influence of therapeutic treatment; and narcotics affect the retina as well as the skin. It may be that a diminution of sensitivity means a diminution of the intensity of movement-impulses; in that event we need not, with Fechner, modify Weber's theory of the sensory circles.

Mind and Body

§12. The materialistic point of view is untenable; it simply puts new prejudice and untruth in the place of old. Fechner carried out all his psychophysical investigations without setting up any hypothesis of the nature of the psychical, and without making any assumption regarding the processes that lie between stimulus and sensation. He suggests, however, in his *Psychophysik*, that physical and psychical may be the same thing looked at from different points of view (cf. the concave and convex aspects of a circle). We may arrive at a like hypothesis by the road of physics. For the facts of physics indicate that we have to do with a finite number of entities and forces, with what are called 'atoms.' Try now as we may, we can frame no rational idea of an 'external aspect' of these atoms; we are forced back upon an 'internal aspect,' in some sort analogous to our own 'mind'. And indeed, how should we ever get to 'mind' at all if the germ of mind were not present in the atom?

Fechner also raises the question of the 'seat' of mind in the organism, and finds it in the whole nervous system. There are difficulties in this view, but they are not insuperable. Fechner further considers the lower animals and the plants, and refuses (rightly) to set any limit for the beginnings of mind. We reached the same result in our consideration of the atom.

In summary, these lectures have tried to teach two lessons: "(1) that physics, physiology and psychology are inevitably bound up with one another, so that each one finds its salvation only in the companionship of the others, and each one may be regarded as handmaid of the others; and (2) that we are not bound to despair of exact investigation when we pass beyond the borders of the palpable."

Herbart and Wundt

Postscript.—Wundt has earned our gratitude by reducing the many riddles of the psychology of perception to a single riddle. But he is unfair to Herbart. (*a*) If the principle had always held good that science cannot be developed out of metaphysical hypotheses, we should never have had a mechanics, to say nothing of a psychology. (*b*) Those who know the Herbartian psychology assure us that it contains much more than could be discovered by mere attentive observation. (*c*) Wundt's criticisms in the *Beiträge* confuse intensity and clearness of ideas. Herbartians themselves do not always escape this error. (*d*) Wundt's experimental disproof of Herbart's two-idea minimum fails: it is as impossible to experiment with two ideas as with two atoms; and Wundt confuses idea with sensation. If Wundt's explanation of his complication-experiment were correct, we should have a mosaic of ideas, but nothing like connected thought.—Herbart has in fact made a very noteworthy contribution to scientific theory. It is no reproof to a young science to say that it proceeds on too simple assumptions. As a rule, Wundt's enquiries begin where Herbart leaves off.

THE DEVELOPMENT OF MEANING

By Raymond Holder Wheeler
University of Oregon

Introduction. The following introspections and interpretations thereof are here presented as a constructive criticism of the recent investigations of Meaning by Moore and McDonough,[1] and as a forerunner of a detailed theoretical discussion of the problem of Meaning.

In an investigation of Choosing[2] it was found that in the act of giving a detailed introspection the observers not only found themselves giving a description of immediate content, so-called, but also found themselves interpreting this content even as it took place. The more detailed the introspection, the sooner after this content appeared did the reagent interpret it. In other instances the reagent did not so interpret his experiences until they had passed and gone.

These facts led to the conclusion that many of our so-called unanalyzable mental processes such as Attitudes, Thoughts, Consciousness of the Self and the like are interpretations, which the observers have failed to analyze owing to lack of training or ability. These facts also led to the conclusion that such "interpretative periods," as these tendencies were called, are legitimate features of any introspection, providing they are properly analyzed and recognized as such.

These "interpretative periods" are no more and no less than the development of meaning. They are complex meanings in that they are definitely analyzable into sensory and imaginal components and involve a definitely describable behavior of attention.[3]

In the experience of the writer it has very frequently turned out that the development of meaning, itself, may assume various forms under the guise of "feelings of meaning," "awareness of meaning," "consciousness of familiar meaning," "recognition

[1]McDonough, A. R. The development of meaning. *Psychol. Monog.*, Vol. 27, 1919, 443-515. Moore, T. V. Image and meaning in memory and perception. *Psychol. Monog.*, Vol. 27, 1919, 69-296.

[2]Wheeler, R. H. An experimental investigation of the process of choosing, *Univ. of Ore. Publ.*, Vol. 1, No. 2, 1920, 59 pp.

[3]See Wheeler, *op. cit.*, 33ff. 51 f.

of meaning" and the like, according to the observer's predisposition to label such experiences in one fashion or another. But just as in the case of the attitudes—acceptance, rejection, surprise, etc.—and consciousness of the self, "awareness of meaning" and "feelings of meaning" are interpretative periods. They are in themselves analyzable and constitute a stage in the development of meaning or in the recognition of meaning when the reagent is assuming an introspective attitude. When the reagent is not assuming an introspective attitude any attempt to describe them fails, for the contents are so numerous and their duration so fleeting that one's attention is entirely unable to cope with the complexity of the situation.

The following introspective data are typical of a vast amount of material of all sorts and descriptions which might be offered in demonstrating that development of meaning need not escape the observer even in its minor details. The introspections are the author's, obtained in the laboratory, with the aid of an expert assistant.

Introspective data. As far as possible, "interpretations" are set off from "descriptions of content" by means of parentheses.

1. *Instructions:* "You will be given a word to which I want you to react as soon as you are conscious of the meaning. Ready, now."

KNIFE. "At the outset I was focally conscious of the word as spoken by the experimenter; attention centered itself upon the long 'i' sound of the word, momentarily, and then shifted to the 'f' quality. These processes had hardly developed when I found myself becoming tense about the vocal organs and in my right arm. Together with these motor reactions there appeared visual imagery of a knife with two blades protruding at right angles to the body of the knife. One blade was smaller than the other but no other details except the steel color and the shape of the blades stood out in this imagery. Then the handle of the knife clarified somewhat and I saw in this imagery the small wrinkles and the varying shades of brown and black which characterize such a handle. This imagery did not develop very far, for at once, breaking into consciousness, was the 'idea', 'cutting instrument'. This idea consisted first of a slight shift of my line of regard from the visualized knife upward and to the right, where I visualized what might have developed into the complete image of a paring knife; but in this latter imagery there appeared only the broad thin blade with the cutting edge upward and shining as if it had recently been sharpened; along the edge were tiny scratches as if made by a sharpening instrument; and at one end of the blade there showed about one-quarter inch of a reddish-brown handle. (In describing the details of this imagery the very use of language makes it seem as if meaning were present when it had not yet developed. I was not aware of the fact, at the time, that I was imaging a paring knife; nor was I aware of the fact as such that the paring knife had recently been sharpened. I have merely been describing the features which stood out in the imagery.) Meanwhile, the motor tenseness which had developed at the outset persisted and my attention was again claimed by it; it was increasing in intensity and was spreading to take in the muscles of my chest and shoulders. (Up to this time the kinaesthesis had constituted the beginnings of a recognition-consciousness, or a "feeling of mean-

ing," or the beginnings of a consciousness that the word 'knife' was familiar.) Just as this motor reaction was spreading I had in vague, fleeting terms the vocal-motor image of 'cut'; but before further verbal imagery came my attention was again claimed by the bodily kinaesthesis. (Here the meaning 'knife-as-a-cutting-instrument' had begun to develop. It seems as if I should then for the first time have been able to say that the meaning of the stimulus-word had become conscious.) The verbal 'cut' then elaborated to the vocal-motor 'cutting instrument.' Here the motor reaction increased still more in its scope. (I was reacting to the 'meaning'; but here the reaction was merely an intensification and prolonging of the kinaesthesis which had already constituted the core of the meaning itself.) I found myself attending to the experimental surroundings, momentarily, as if the task had been completed; relaxation tended to set in, but this was inhibited by the development of further meaning. The 'idea' occurred to me that 'knife' might mean an 'instrument used in eating.' This is an interpretation which followed a flashy visual image of a silver eating-knife poised as if in someone's hand but with no hand visible; it was located in front of a very diffuse and vague white background which I later interpreted, vocal-motor fashion, to be a table-cloth. The class-meaning 'instrument' was not present to consciousness as such except in the vocal-motor tendency to define the knife; this latter tendency appeared at once upon the appearance of the visual imagery of the eating-knife, and gave way at once to an awareness of the instructions and to the *Aufgabe*-consciousness to begin to introspect. At no time during the experience was there any meaning present to consciousness as such other than in imaginal or in kinaesthetic terms."

2. *Instructions:* "You will be presented, visually, a word to which I want you to react after you have mentally defined its meaning. Ready, now."

PAIN. "I reacted at once to the meaning 'sensation, pain.' As my line of regard fell upon the printed word I perceived it at once as a whole, and before I became conscious of the fact that I had thus perceived the word as a whole there developed a muscular jerk about the left shoulder and a tenseness in the throat. (This 'jerk' constituted a feeling of meaning.) Momentarily the quality of tension stood out focally in consciousness, while at the same time I was still, non-focally, perceiving the stimulus-word. (This broadening of the span of attention to take in the stimulus-word and the motor reaction made it possible for me to say that it was the stimulus-word which seemed to possess the meaning. The kinaesthesis was the content of the meaning-consciousness itself.) Then, as this 'jerk' persisted, I had a tactual image of pain so brief and fleeting that it was not at the time localized; it was merely a pain quality, punctiform and sharp; but almost as it came, it disappeared. Momentarily, and after the pain-image had vanished, I tended to visualize the outer surface of my left shoulder as if I were now trying to localize the pain. Along with this I was vaguely conscious, in terms of visual imagery, of my bodily position and of my line of regard as it was extended in the direction of my shoulder. All of this happened before the muscular 'jerk' in the shoulder disappeared. (This latter tendency to visualize the entire situation constituted an interpretative period, an instant in which I was conscious of the fact that meaning had developed. In other words the kinaesthesis itself in the presence of the pain-image constituted the meaning 'sensation of pain,' while the visualization of this kinaesthesis constituted my consciousness of the meaningfulness of the entire experience. When the kinaesthetic 'jerk' became supplemented by visual imagery and the broadening of the span of attention, the interpretative period had developed. There was positively no meaning present over and above what I have described.) I then found myself relaxing. But before this relaxation had completed itself I thought

of another meaning. The stimulus-word then meant 'window-pane'. This developed as follows: I found my line of regard shifting upward and to the right from the visually perceived word, 'pain', and here there tended to develop vague visual imagery of a window, the only features of which that stood out were a blue-grey, smooth surface, translucent but not transparent, as if it might have been ground glass; but I was not conscious of the fact at the time that it might have been ground glass. This consciousness comes only in the telling of the experience. This glass, in my imagery, was surrounded by sash, on two sides, about an inch wide; the sash was dark, with a very faint tinge of brown. My visual image did not take in the left or bottom edge of the pane. My visual attention was centered upon the blue-grey quality of the pane and upon its smooth surface. At that time I was not aware of any verbal tendency to say 'pane,' but immediately after this visual imagery had flashed into consciousness I found myself saying 'window-pane,' and as I did so my visual attention increased in scope to take in the distance between the projected pane and my head. (This latter experience constituted a recognition of the meaning of the experience—an interpretative period. The development of the first meaning in connection with the stimulus-word took place so rapidly that it was all over before my line of regard had swept across the word from left to right. The final meaning developed almost as quickly, and came during the last stages of my motor reaction. The background of kinaesthesis became the vague content of my final interpretation period, and it was toward this kinaesthesis that my attention was spreading during the interpretative period.)"

3. Same instructions as before.

SCULPTOR. "I had hardly perceived the word, visually, before I found myself reacting to the meaning. My line of regard fell at the outset upon the first four letters of the word, and before it had shifted far enough to the right to see the remaining letters in focal vision I filled out the word in visual imagery of the remaining letters, together with vocal-motor imagery of the entire word. The latter was exceedingly clear-cut and vivid, especially the imagery of the throat and lip positions in saying the 'tor' portion of the word. Together with this emphasis upon the 'tor' there appeared visual imagery of the 'o'. Along with all of this imagery there suddenly developed tensions about the chest, shoulders and in my right arm. I was already beginning to react. (This kinaesthesis constituted a "feeling of the meaning"—a recognition of the word as a familiar one; but as yet there was nothing in consciousness that involved the concrete meaning of the term. At this juncture I was not aware of the meaning as such. So far the processes might be summed up in the phrase 'I know the meaning of the word.' Neither did this response itself mean familiarity as such; it meant familiarity in virtue of an immediate tendency to review my experiences thus far and to label them. This was done in terms of a tendency for the span of attention to widen and in a tendency to visualize the kinaesthesis and the word itself at the same time.) Then my line of regard shifted in imaginal fashion to a region directly above the printed word; there I visualized a man, very indistinctly and only in part. Here I labelled no features at the time; I am describing only the details of the imagery. I saw the right half of a square jaw, a sunken cheek, very dark skin, wrinkled in such fashion as to indicate that it was very coarse and rough; above was a glimpse of shaggy, frizzled hair; below was a glimpse of one side of a worn, dirty, shaggy coat and of one trouser-leg, dirty, unkempt and badly in need of pressing. The coat and trouser-leg were black. Off to the left these features merged into indefinite blackness and from hence into nothing; to the right this imagery merged into an undifferentiated area of grey and brown—what might have developed into the details of a room.

I then noted in this imagery that the man's right arm was outstretched; I found myself tending to look in the direction of this outstretched arm, but did not actually do so; I was merely aware of incipient eye and neck tensions of turning in that direction. During this time my motor reaction had persisted and had become more intense, constituting a reaction to the entire situation—the stimulus word and the imagery. (Again I was reacting to familiarity and to but the beginning stage in the development of definitized meaning. Up to this time the meaning was 'sculptor as an individual'; and the more fully developed meaning came only after I found myself reacting.) I was then conscious of a tendency for my line of regard to shift suddenly and vigorously to the right of the stimulus-word; there I found developing a visual image of something tall and brown; it did not become definite enough to describe in detail; it had three 'lobes' or sections as if it might have been the beginning of a visualized group of statues of three persons in bronze. This vague, fleeting and undifferentiated visual image rested upon a vague, dark 'something' below, and was projected in front of a dark green background, which latter had no definite limits in any direction. All of this latter imagery was accompanied by a sudden tendency for tensions to develop in my chest, arm and throat, and this tendency was distinctly perceptible over and above the antecedent movements which had to do with reacting. I then found myself visualizing the 'tue' part of the word 'statue,' and was aware of the word 'statue' tapering off in consciousness; I had failed to detect this latter imagery as it had entered consciousness. (All of this latter imagery, together with the motor response, constituted a recognition of the meaning, 'statue,' and was an interpretation period.) Then I had, rushing into consciousness, visual imagery of the head and neck of a horse together with the anterior portions of his shoulders; I was not aware at the time that it was a horse or that it was a marble horse. Brightness, form, shadows, the curve of the neck, jaw, the bony protrusion above the eyes, the forelock, were all featured in the imagery, but the whole experience lasted for only the smallest fraction of a second. (This imagery was the first stage in the development of further meaning—sculpture.) The last feature of the visualized horse to stand out in consciousness was the wrinkled musculature along the anterior-lateral portions of the near shoulder. As my line of regard was leaving this image and as the image was becoming indistinct, I again found vocal-motor imagery tapering off in consciousness. This time it was the vocal-motor 'sculptor'. Again I had failed to note the image as it had appeared. As this latter awareness of verbal imagery developed, part of it spread to a tendency to visualize my throat and the space between my throat and the vanishing image of the horse. (This latter experience constituted an interpretative period. I had now mentally defined the word 'sculptor'.) During the development of the two meanings, 'statue' and 'sculpture', there were two distinct motor 'sets'; these 'sets' differed slightly. The first involved slight eye-tension of holding my line of regard upward; the second involved eye-strain directed toward the right; the first was not as intense as the second; the first was confined more largely to the throat, chest and eyes, while the second involved incipient movement of my right shoulder as well. Both 'sets' constituted a background for the developing visual imagery and seemed essential to the development of meaning. It was to these kinaestheses that I invariably found myself referring the meaning itself. At no time during the entire process did I have any affective toning."

4. Introspection on the development of an illusion and its subsequent change in meaning: experienced as the reagent was about to enter the Psychology Building, and taken down at once upon reaching the Laboratory, less than five minutes later. In the meanwhile notes had been scribbled on paper.

"As I was walking toward the laboratory building I was very dimly conscious of my surroundings except now and then of a momentary visual perception of the walk in front of me. Passing focally through consciousness were trains of verbal images such as 'are the corrections too numerous', 'will the printer object', and the like. Together with these verbal processes there appeared fleeting visual images of Mr. H., the chief of the printing establishment, and visual images of presses, cases of type, etc. I saw Mr. H. in his familiar dark grey suit; he was standing opposite at a table and was slightly stooped over; his head was turned downward and slightly to the left; I did not visualize his face distinctly; I did not visualize the whole of the table, but saw its dark brown color, the thickness of the top, and a mass of papers scattered about. None of these features became clear during the brief existence of the imagery. (I seemed to be in the printing room as if I were talking to Mr. H. about the manuscript which I was actually holding in my hand, and saw these details as I should have done had I been at the table opposite him.) Then there appeared a mass of visual imagery of different pages of the manuscript. These were but fleeting glimpses particularly of such pages as were considerably marked with corrections. I then found my attention centered upon several words which began with 'w' and which I had retouched with ink. While my line of regard was thus skipping about from 'w' to 'w' and had reached midway down a visualized page and a little to the right of the center, I had the very fleeting and syncopated vocal-motor-auditory: 'Is there someone behind me?' in rising inflection. Up to this time the tactual-kinaesthetic sensations from walking had remained in the dim background of consciousness, as well as a kinaesthesis of frowning, and tensions from the bowed condition of my neck as I walked along in deep thought. At this juncture these kinaesthetic processes began to approach a higher degree of distinctness; then there suddenly developed the motor processes of turning my head, shoulders and upper trunk to look behind me. At this time there was a change in the 'set' of my facial muscles, but this was so diffuse that I can not describe it in detail. There was in this change a relaxation of forehead-muscles from the frowning condition of the moment before. Then rushing into consciousness came visual perceptions of my surroundings—the walk, the green lawn on both sides of the walk, near and distant trees, etc. As my line of regard swept over the distance between me and the trees and adjacent buildings I perceived more and more of the sky in the background, although in indirect vision. This entire change of scene took place very rapidly while I was turning round, but nevertheless the successive focussings of attention upon the near and then upon the farther objects were easily detected. By the time my line of regard was focussed some distance down the walk, and with my eyes centered upon the walk itself, I 'saw' in the sky, above the trees and out over the town several blocks distant, a huge electric sign, in indirect vision. Here my imaginal line of regard was centered upon the first letter of the upper left-hand word, which was clearly a 'W'. The remaining letters of the word and the other words as well were hazy and indistinguishable. But my imagery included the entire sign which had three rows of words, one row above another. It was a typical large electric sign in that the letters were of uniform size, large and blocked; the letters were black, but around them and behind there were faint suggestions of yellow, the yellow forming the sides of the block letters. Surrounding the entire sign was the blue-grey background of the sky. At the instant this imagery appeared and while my line of regard was still focussed upon the 'w' there developed a sudden kinaesthesis of surprise, consisting of tendencies for the musculature of my shoulders and neck to become fixed in a rigid position—a marked change from the gradual movement of the moment before, of tendencies to hold my breath and to droop my lower jaw, of a sudden circulatory change which appeared as a tightness or pressure in the left side of my chest, and of a wave of pressure welling

upward toward my throat. Along with this attitude of surprise there appeared syncopated verbal imagery of "Westinghouse Elec—." I cannot remember that the words were completed. Then my consciousness became focally visual again, and I found myself tending to visualize the words 'Westinghouse Electric Company,' but this imagery was very sketchy and fleeting and only a few letters here and there stood out. This imagery was localized below and to the right of the visual image of the sign and much nearer me. During the appearance of this latter verbal and visual imagery there was a decided change in the muscular 'set' of the moment before; tensions from the attitude of surprise became much less intense; momentarily I was aware that the sign 'meant' Westinghouse Electric Company. This awareness of meaning consisted, first, of a non-focal relaxing of tensions particularly about the neck and shoulders with visualizations of these regions; secondly, of pleasantness which I am totally unable to differentiate from a momentary relaxed condition of muscles about the mouth—a kinaesthetic consciousness; this region was also visualized as I became aware of the kinaesthesis; and, thirdly, of a very diffused bodily set which might be described as 'acceptance' but which did not mean acceptance to me at the time. This attitude consisted of a more general awareness in kinaesthetic terms of my whole trunk, my head and my neck. This was a diffused, relaxed condition, hardly more vivid in one region than another, but distinctly involving a wider region than at first and a decided change from the general muscular 'set' of the moment before. All of this was visualized and was accompanied by a 'set' in the throat as if to make some remark, with the vocal-motor 'I'. (This latter and very complex motor 'set' developed at once upon the appearance of the verbal and visual imagery described above. It constituted an interpretative period—a recognition of the meaning as such.)

No sooner had all this happened than I found myself turning back to pursue my way to the laboratory. The kinaesthesis of turning was hardly perceptible. I had no sooner turned than I again found myself looking back, this time directly toward the region where I had just 'seen' the sign. The transition of objects across the field of vision was again noticed, but this time non-focally. Consciousness was now occupied with the kinaesthesis of turning and with a wonder as to what the experience was all about. This wonder consisted of persisting throat tensions, in the suddenness with which I turned about the second time, and of a questioning consciousness, which latter was characterized by a focalized kinaesthetic strain in the neck as I turned my head around, upward and slightly sidewise along with the movements of turning my shoulders and trunk. There was also faint auditory imagery of something, too indefinite to describe. It was in terms of my own voice and sounded like 'Uhm'. Also there lingered in the background of consciousness the tightness of circulation in and about my chest and neck. As soon as my line of regard was directed into the sky toward the place where the sign had appeared, there developed very suddenly and with great focality a visual perception of what I had previously interpreted to be an electric sign—clouds of smoke. Here the meaning 'smoke from a factory' developed with a degree of vividness which was evidently rendered all the more focal in contrast to the meaning 'electric sign' of the moment before. The first feature of the visual perception to stand out focally was its brightness; attention was centered upon the brightness-quality of the smoke as if in contrast to the blackness of the letters of the moment before. (This statement about contrast is but an interpretation based upon the suddenness with which the brightness-feature of the smoke developed into consciousness.) This behavior of attention was only momentary, for the yellowish tinge of the smoke then stood out focally, accompanied by a non-focal verbal image of the word 'yellow' and a concomitant tendency to visualize a large yellow 'Y' very dimly localized upon the smoke itself as a background. This tendency was so fleeting as to

be almost overlooked. The next features of the smoke to stand out were its motion and its rolling cloud-like formation as it floated across the sky, spreading and becoming thinner as it did so. These details then gave way to incipient tendencies to follow the smoke down and to the right toward its origin, but this motor tendency, together with its shift in line of regard, had but just commenced when I found myself anticipating the source of the smoke by means of visual imagery of a black iron chimney and a red, flat-roofed building. With this imagery there appeared incipient verbal imagery of the word 'gas', and then 'factory'. Then at once a general bodily 'set', much like that described above, developed. (At this time I was not conscious of a difference in meaning nor was I conscious of a similarity in the two 'sets'. These facts are after-thoughts. The confusion comes in being forced to short-cut the introspection by drawing similarities and differences as one introspects. After the experience was over I became conscious of the differences of the two meanings and of the similarities of the two 'sets'.) This second motor 'set' consisted of a very general, wide-spread, intense 'jerk' or tenseness which began in my throat, spread to my face, and then down into my chest and about my shoulders. (I think that this is merely the order in which my attention took in these kinaestheses; it is a sort of introspective analysis which was going on at the time; for just previous to this behavior of attention the strains had developed but were so general and diffused that none of them was localizable nor were any of them definitely describable. They became describable after my behavior of attention toward them seemed to result in their definitization and localization.) Again these tensions seemed at first to be nothing more than a changed condition from the kinaesthesis of turning around. At this time the musculature was not visualized and there developed no attitude of acceptance and no interpretative period; I found myself tending to label the experience immediately afterward, verbal fashion, as the development of meaning. No intervening processes took place other than a tapering off of the kinaestheses and visual imagery from focal attention. This interpretation was a visualization of the region of my throat, together with a tendency to innervate the word 'meaning' and a still further change in the general motor background. This latter change consisted of localized relaxations about the mouth and eyes, while attention took in non-focally a general awareness of the 'resident' bodily kinaesthesis elsewhere, and also a kinaesthetic-static image of lifting myself upward. This was accompanied by dim visual imagery of my body from the trunk up. Much of this imagery and many of the sensory experiences were dim and fleeting. The latter group of experiences, constituting a tendency to label the entire last half of my reaction as the development of meaning, ended the entire affair. (Here the interpretative period was delayed; but we notice the same tendency for attention to broaden its scope and to take in kinaestheses *plus* visualizations of musculature.) Nowhere in the entire experience was there any hint whatever of imageless or sensationless meaning. On the contrary, I was distinctly conscious of the fact, immediately afterward, that images and sensory processes constituted the meaning."

Discussion of data. The reliability of such introspections as are given above has been checked repeatedly in the laboratory by means of giving two consecutive introspective accounts of the same process. In no instance have elaborations or important omissions been found. Constant experience over a number of years in giving introspective descriptions of complex experiences has convinced the writer that, elaborate as they are, the descriptions are faithful to the facts.

It is obvious that as far as this reagent is concerned there are no unique meanings nor are there any 'superstructures' of meaning unless one wishes to regard interpretative periods as 'superstructures.' But these 'superstructures' themselves are analyzable and are invariably sensory or imaginal in their content. Bodily kinaestheses added to vocal-motor and visual processes change implicit into explicit meanings. Meanings seem never to be fully developed until the original contents of consciousness which function as the 'process-aspects' of meaning lead to an interpretative period. Without these periods meanings are implicit; they exist only in the behavior and context of original contents. With these interpretative periods the meanings become explicit; they become data of consciousness; whereas before the meanings were only data in consciousness. By means of these interpretative periods the reagent becomes conscious of meanings.

The above introspections are full of 'thoughts,' 'feelings of meaning,' and 'conscious attitudes,' yet none of them reveals the slightest evidence of meaning in the absence of image or sensation.

It is evident that, before an introspection upon meaning is complete, at least three stages must have been analyzed. (1) The reagent must describe what is known as 'original content.' This includes a statement of the imagery and sensation components present in consciousness. Such a procedure is possible only when 'immediate content' is reviewed by assuming an introspective attitude. In other words these original contents must lead to further contents before the former can be described, or before they can become data *of* consciousness. (2) Insofar as meaning is involved in original content it is implicit. By means of an interpretative period the reagent becomes conscious of the meaning. That is, the meaning becomes explicit. The introspector must analyse this interpretative period, else he will be projecting the consciousness of meaning back into the original contents where the meaning itself was not conscious at all but merely implied in the context and behavior of contents. This interpretative period, in this reagent's case, invariably includes a peculiar behavior of attention in which verbal and visual imagery are tapering off and in which kinaesthesis comes to the foreground and is visualized. (3) This second stage must lead to a third before the reagent can become conscious that the consciousness of meaning is an interpretation. This takes the form of a secondary interpretative period or the form of translating the experience of meaning into the language of introspection.

Where Stage 2 does not develop, the reagent is not conscious of the meaning, but in virtue of Stage 3 he is conscious that

the experience *was* meaningful and refers the meaning to kinaesthesis.

In Stage 1 appears a bodily 'set' along with verbal, visual, or auditory imagery. Imaginal content functions as the stimulus to which the bodily 'set' seems to be a response. Both groups of factors are essential for meaning. Stage 2 appears in terms of this lingering bodily 'set' while the stimulus-imagery is tapering off, and this 'set' is visualized. Attention here broadens to take in the space between the localized stimulus-imagery and the localized kinaesthesis. Both of these groups of factors are essential to Stage 2. Stage 3 includes this same kinaesthetic set, still visualized, but tapering off in consciousness, as one factor, and a verbal tendency to label the experience as meaningful as the second factor.

In each stage, therefore, two sets of factors are essential—a stimulus function and a response function.

Conclusion. The development of meaning involves three stages: (1) the original or 'given' process; (2) a subsequent process which interprets the first; (3) a third process which constitutes a final interpretation. The first stage is the original content of consciousness; the second stage is necessary before this original content can become a datum *of* consciousness; the third stage is essential before the meaning of this datum can become a datum of consciousness.

In other words there are (1) shifting or developing sensory and imaginal contents, and these must shift or develop before their antecedents have finally vanished from consciousness. Along with this development of sensory and imaginal contents there arises a motor 'set' or attitude. An image or a group of images in a context never 'meant' anything in the absence of this motor 'set' or of verbal imagery. (2) These data develop by means of a peculiar broadening of the span of attention to the stage of consciousness of meaning. Here attitudes become conscious attitudes. (3) The motor 'set' undergoes certain changes in emphasis and is supplemented by verbal imagery and further visualization of musculature. This constitutes an awareness that the meaning has been or is now being recognized.

One stage develops suddenly and with no line of demarcation from its antecedent. Stage 2 develops before the contents of Stage 1 have vanished and is a behavior of attention by which we 'know' what these contents are and by which original data *in* consciousness become data *of* consciousness. Stage 3 is a behavior of attention by means of which this knowledge becomes explicit rather than implicit, *i. e.*, by which a recognition is recognized.

Stage 1 consists of process-aspects of the so-called 'given' mental contents and meaning is absent until kinaesthesis appears. In every stage kinaesthesis is the core of the phenomenon which we call meaning. In every stage there is a shift in the emphasis which attention puts upon the contents themselves. It is quite evident, therefore, that kinaesthesis is vital to the development of meaning. In the writer's case, at least, meaning has not been described until kinaesthesis is taken into account. It is quite probable that pure meanings, so-called, are in reality masses of diffuse muscular sensations which the reagent has not succeeded in recognizing and in describing.

CREATIVE IMAGINATION

By Ray M. Simpson, A.M. (Harvard)

I. Importance of the Subject

"The blunt truth is, that after three years' association with amateur writers, we are convinced that only a small percentage are naturally gifted with the two vital elements of success—creative imagination and dramatic insight." This quotation, taken from a letter received from Mr. Roy Manker, Vice President of the Palmer Photoplay Corporation, shows the great importance that a business man attaches to creative ability.

Creative imagination is worth more than mere book knowledge. Education and intelligence are merely the means by which we facilitate the liberation of this creative energy. Modern psychologists have failed to place the proper emphasis upon this vital and important factor in the life of the individual and in the fibre of our society. Titchener has been the most generous in offering suggestions upon the subject, as is evidenced by the fact that he has devoted three pages to the imaginative consciousness in his Text Book on Psychology. Wundt speaks of "Creative Synthesis" and "Creative Resultants," which are most fertile as suggestions, but which he drops with his customary propriety. The Behavioristic School naturally overlooks imaginative tendencies. Watson in his latest book on Psychology does not even mention the term "imagination."

It is not our purpose in this article to ascertain how imagination originates, but to deal only with ways and means of measuring it. Certain types of incidental or promiscuous education may develop it more readily than specialized training. The extraordinary produces the extraordinary. Productive freaks are the exception and not the rule.

Tests devised to ascertain either native intelligence or acquired knowledge are certainly valuable to an employer. However, they can only give a clue to the individual's possible expansion in so far as certain tendencies and capacities have been seen to evolve from other individuals possessing the same intellectual status. Individual differences make the index of these tests most uncertain. There are no elements in them to extract from the mind of the individual his powers of creative

productivity and his tendencies toward originality. If his creative ability is expressed in many of these tests, the methods of scoring have failed to take it into consideration. It is evident that we need tests designed to give us more direct and dependable information upon this essential element of progress—creative imagination.

II. How Creative Imagination May be Detected

Creative ability is marked by the initiative which one evidences by his power to break away from the usual sequence of thought into an altogether different thought. Progress is made by the realization of limitations. When these limitations are recognized, then a new approach is sought for an explanation of the problem confronting us—or should be sought.

Frequency of spontaneity in thought is the true measure of a person's creative capacities. A person might be pregnant with new combinations of ideas, and yet they would not have any special significance as being useful, artistic or unique. We are looking for deviation, not necessarily the utility of the creation or the beauty of a design. Creative ability is evidenced in one's tendencies to abandon old unfruitful paths for others. A searching type of mind, a combing mind, a synthetic mind is what we are looking for.

The number of figures drawn within 15 min. is no index of a person's creative imagination. This gives merely his speed in production. It has no quantitative or qualitative significance. The number of departures from following a previous figure in its form or utility, however, marks that quality of mind that is original and searching.

Initiative of deviation is for our purpose, then, accepted as the measure of a man's creative ability. This deviation may be either evolutionary or spontaneous. It may involve either mechanical production or inspired production. People vary as to initiative. The nervous individual, possessing aesthetic tastes and sensitive feelings, may possess more initiative or liberate more energy than the indifferent calm thinker. But initiative towards one's task is certainly no mark or evidence of constructive ability and originality. It is true that this may be a valuable asset to a creative mind, however, because of the facility which it engenders.

Now, what motivates these deviations? Ambition, that intangible incentive to action which looks forward to satisfaction, is absolutely necessary. Hedonistic motives no doubt exercise the predominant developmental urge.

All creations, such as novels, inventions, poems, etc., may be placed in one of the two categories—labored or inspired. Some

geniuses flower in a day, while others are destined to years of hard, tiresome labor and effort before their dreams are realized. We are walking along the street and an idea suddenly springs into our consciousness. It is new in the sense that it is unique. There is never anything absolutely new created. New ideas represent merely new combinations of thought-elements, of which there is an infinite number of combinations. Maxwell Garnett, in his new book on "Education and World Citizenship" (1921, p. 258), gives the following interesting paragraph dealing with this point. "And, since these sudden inspirations often follow an unconscious process that has revealed a connection (or, as Poincaré says, a 'combination') which the preceding period of conscious work failed to disclose, are we to say that there is an unconscious ego, or a subliminal ego, that is 'superior' to the conscious ego? (Both Poincaré and Dr. Morton Prince are loth to say yes.) Are we to follow Mr. Kenneth Richmond and describe the thought processes which lead to these inspirations as 'superconscious'? It is true that creative thinking, marked by the 'discovery' of valuable combinations and so making important progress towards the goal of scientific thought, is often—and perhaps only—accomplished when active consciousness is somehow side-tracked, when, in fact, the will is not intervening in the train of thought in question, and when there is no interference by extraneous thought-activities, whether due to active interest-systems or to in-coming sense-impressions." Garnett attributes our moments of inspiration to the free working of the curiosity or wonder instinct. It is true that interest may motivate our inspirations, but the inspiration itself is dependent upon many other factors besides interest.

Why are we justified in considering creative capacities as dependent upon frequency of spontaneity, initiative of deviation or absolute change? The methods and productions of all persons whom we designate as geniuses give ample proof of this assertion. Newton, Faraday, Becquerel, Roentgen, Darwin, Einstein, Edison and Madame Curie give us a representative list of scientists who certainly differ from the ordinary individual in creative originality of production. These inventive geniuses possess what we desire to call Creative Imagination. It may be applied with equal significance in all the fields of human endeavor. Mr. Arthur Hopkins' recent production of "Macbeth," with the new conceptions of stage setting as devised by Robert Edmond Jones, certainly is radically different from what we have usually experienced. Mr. Jones is a creative genius. Circumstance and environment often hinder as well as facilitate the functioning of creative ability, but cannot smother it.

III. Genius *vs.* Insanity

Our method of calculating creative ability may at first seem to signify that insane persons would be the more creative. This is contrary to fact. In our estimation there are two types of disassociations, (1) creative and (2) neurotic. They differ in quality and degree. One may possess both. Lombroso's contention that genius and degeneration go hand in hand is obsolete and untrue. We prefer to refer to the similarities and speak of them as mere resemblances. "Genius resembles insanity as gold resembles brass" (Hirsch). Resemblances do not prove interdependence. At present we are planning to carry this same experiment into Psychopathic Hospitals and to determine the correlation between the normal individual and the neurotic individual with reference to their creative capacities.

Havelock Ellis tells us that "nothing in British Genius seems favorable to Lombroso's favorite theory, that genius occurs upon an epileptoid basis." "Some writers rake together cases of insane men of genius without considering what proportion they bear to sane men of genius, nor what relation their insanity bears to this genius."

The new combinations of thoughts of the insane have no significant bearing upon their past experience. Twelve deviations as expressed by a neurotic individual have an entirely different significance from the same number expressed by an individual with a sound mind. The two types fall under entirely different categories. The normal mind possesses the power of organization and the ability to realize the utility and function of its ideas while the insane person does not. Biography has warped the facts in the majority of cases of recorded insane geniuses. Freaks and seclusive types were formerly adjudged as insane. The meaning of the term has changed radically within the past two hundred years. The outstanding geniuses of today are far from being insane (Marconi, Voronoff, Edison, Carnegie). Let us hope that the influence of modern psychological methods will exact more careful consideration of mental traits in biographical records.

It is essential to record the "stability" of the individual with the results obtained by any test upon that individual. We proceed next to the test itself.

IV. Test for Creative Imagination (Visual)

Purpose. To determine the creative capacity of an individual.

Material. Four small round dots, representing the four corners of a square which measures ¾ in. from dot to dot, are printed upon white paper. (It is best to have a rubber stamp made in order that they may be uniform in each figure.) Five squares are printed upon an ordinary sheet of 8½ x 11 typewriter paper. Leave ample space about each square.

Preliminary. (For a group.) Pass out the printed material before starting to explain what is to be done. Give each individual 10 sheets of paper (50 squares). Have him write his name, age, and occupation in the upper right hand corner of the first page. If school children, have them give their grade in school. Have them lay papers aside while you give the directions.

Directions. (For a group.) "Upon each paper that I have given you there are printed several groups of dots in the form of squares. When I say 'Go' you are to add two more dots to the four, which you have in each of the printed squares, and see how many different drawings of objects, or of designs, you can make in fifteen minutes. You may place the two 'extra dots' anywhere you like. You must use every one of the six dots. You can make as many straight or curved or crooked lines as you like. Use pencil.

"Do not be too careful with your drawings. The objects must be intelligible and the designs or patterns must use the two 'extra' dots in important places in them. We know that some of you can draw better than others. That makes no difference. Originality counts for most. Strive to make as many *different* things as possible. You are searching for 'new' ideas. Try to make every one of the drawings *different.*"

(Give two examples upon the black-board, showing how a dipper and a rectangle can be made by adding two dots and connecting them by lines. Be sure to make the dipper with a curved handle in order that they may see that curved lines can be used. Make it clear that they can use any kind of crooked lines.)

"Number each one of the drawings as you finish them. Remember to add two more dots to the four. You will have fifteen minutes only. Pay strict attention to your own business and do not look at your neighbor's work. The best way to find things is to keep looking at the four dots.

"READY! GO!"

(Watch the time closely. Be sure they have numbered the drawings before they hand them in.)

Questions and additional data:

1. Now some people place the two dots "by chance" anywhere within or outside of the square and then "imagine" what the outline suggests. Others first think of the thing they are going to draw, then place the two dots where they are needed to complete their picture or design. If the *thought* of the object you drew came into your mind before you placed the dots, write Yes on the back of the first page. If you used both methods write Both.
2. Write beside each figure what suggested it to you.
3. Did any of the things that you drew before have any influence upon your other drawings?
4. Did you think of the next figure that you were going to draw before you finished the previous one?
5. Which do you think is the more "creative" or original—the drawing of a geometrical wall-paper design or the drawing of an automobile?

V. HOW TO GRADE THE DATA

Give one point credit for a change of design in decorative drawings and one point credit for a change of object in representative drawings. By noting carefully the form, function, utility, shape, pattern, setting and meaning of each drawing it will be very easy to make these two main distinctions. Do not count a reproduction of the examples that were given before the experiment. Count only one letter of the alphabet and only one figure.

Geometrical figures count the same as any decorative design or representative object. The frequency of geometrical figures is about the same in all individuals. In talking with Professor Titchener about the comparative values of "designs" and of "objects," he stated that he was not certain but that the drawing of a design was more original and creative than the drawing of an object. We will not attempt to discuss this point here. We are inclined to think that they are of equal value. The artist may have a large supply of "stock" designs on hand to unload, but he is restricted to the use of only those which can utilize the two extra dots to advantage. A blacksmith, on the other hand, may have in readiness a supply of objects which he experiences with equal frequency in his work, but they are subjected to the same requirements in this test.

The total number of drawings made is not significant. If a person drew forty houses all practically alike he certainly would not be considered as "creative". Another may draw only five designs each of which has the same shape or form. We would score each with a zero.

Suppose Mr. X. made six drawings which were entirely different (man—house—boat—flower—fylfot symbol—butterfly), and Mr. Z. made thirty drawings of which only fifteen were entirely different. Mr. Z. would be considered more creative because he has given us fifteen changes while Mr. X. has given us only five. But what of the quality?

How are we to determine which is of more creative significance —a typewriter or a Rembrandt painting? How are we to differentiate between the creative abilities or creative capacities of a Goethe or a Schiller except by merely designating them as either active or passive? Who is the greater genius,—Plato or Bach? We are not dealing with qualitative distinctions in this test. We merely hope to ascertain quantitative creative values. The difficulties to be confronted in either case are extremely complex. The social significance or utility of a patent determines its value. We cannot go into a thorough discussion of this matter here, but will make that clear in a later paper. Quality depends to a great extent upon conditions external to the individual, upon education and environment. We are not testing the "product" but the "capacity" to produce original ideas. The fast thinker gives evidence of his speed in the greater number of figures that he draws, but bear in mind that there is greater chance of having many drawings alike where more are made than where few are made. College students in the Harvard Laboratory tended to make fewer figures and to get them all different, while the children in the grades draw a greater number of figures and get probably only half of them different.

Speed gives us no evidence. It merely signifies a difference

in method in creative production. While the fast thinker is drawing three or four similar drawings that only count for one point credit, the slow thinker is generally producing one figure that also counts for one point credit.

At first we attempted to collect the total number of figures made and unite it with the number of creative changes, stating both in a percentage. To secure this percentage we divided the number of creative changes (C.C.) by the total number of figures drawn. According to this method of figuring, an individual who made 5 figures, 3 of which were entirely different (60%), would make the same score as another who made 30 figures with 18 changes (60%). Here we have three creative changes in one individual equal to eighteen creative changes in another. Suppose Mr. A. made 15 figures with 14 changes (93%); Mr. B. made 22 figures with 21 changes (95%); Mr. C. made 15 figures with one change (6.6%); and Mr. D. made 22 figures with one change (4.5%). One can readily see that the value of the changes is here dependent upon the total number made. Our emphasis is upon the "changes", not upon the number of drawings.

VI. Value in Schools

In April (1921) this test for Creative Imagination was given to 407 children in the Public Schools of Oyster Bay, Long Island. Mr. B. E. Whittaker, Supt. of Schools at Oyster Bay, gave generously of his time and energy in aiding with the testing. The tests were given first in the eighth grade and then carried successively through the intervening grades down through grade 3 B. We started with the eighth grade in order to familiarize ourselves with the procedure before we approached the lower grades. Every effort was made to be sure that the procedure was well understood by all the pupils before starting. Language was adopted which they could understand. No essential deviation was made from directions as given above.

Following is a brief summary of the results obtained.

Grade	No. of pupils	Total No. of Figs. Drawn	Av. No. of Figs. Drawn	Total Creative changes	Average C. C.
3B	45	965	21.4	196	4.35
3A	39	721	18.4	276	7.05
4B	38	652	17.1	174	4.5
4A	39	734	18.8	379	9.7
5B	40	1067	26.6	349	8.7
5A	37	703	19.0	352	9.5
6B	34	578	17.0	372	10.9
6A	38	779	20.5	429	11.28
7B	36	566	15.7	349	9.6
7A	30	542	18.06	295	9.8
8	31	421	13.5	190	6.1
Total	407	7708		3361	
Average			18.9		8.25

These pupils ranged in age from 7 to 16 years. Each of the 407 tested had studied drawing. The finding which was of most importance to us was the average number of creative changes for the entire school. This average was 8.25. By comparing the number of creative changes of each individual with this average we have a basis for comparative judgments. Anybody who falls below this average is considered low in creative capacity, while anybody who ranks above 8.25 is rated accordingly.

In every grade, room A proved to be more creative than room B of the same grade. This seems to signify that Oyster Bay has a well graded school. Grade 6A has the highest average, with 11.28 creative changes. The budding genius of the 407 pupils was Edward M., 13 years old, in the sixth grade.[1] He drew 42 figures and had 27 perfect changes. Thirteen of his drawings were of objects.

These tests serve to indicate in a concrete way the latent capacities of each pupil. The task of the teacher then is to study the "interests" of the pupil and place him where he can develop his talents. If he is stupid in arithmetic, then try him in history. If backward in history, then try him in music or drawing. All must undergo a certain amount of the constraint of the school, however. It is evident that Edward M. possesses this "vital stuff" of life which we call "creative energy" and it is the duty of the teacher to direct its liberation into fruitful channels.

Those below the average can be of value to the community only as "reproducers" and probably will fare better in life than those above the average in creative imagination. Those above this average we term "producers"—those who bequeath something to civilization in services. The average business man who is successful may possess very little "creative imagination." True business ingenuity, however, lies in one's powers to visualize situations and in the creative impulse to react to productive opportunities.

VII. Development of the Test

At first we used 15 dots placed promiscuously upon a sheet of paper, and instructed the subjects to find as many "detailed objects or designs" as possible among them within 45 minutes.

[1]Mr. Whittaker says: "Many of those who made high scores may be classified as day-dreamers. Edward M. is about a 75% or 80% student in the usual school subjects. He is a special type and his case is difficult of analysis. He seems to have no strong likes or dislikes and no prominent interests, is not a movie fan, is not inclined to join with the other boys in vigorous plays, is not a great reader of stories or novels. Yet he has a very active imagination, exemplified in the peculiar kinks in his own expressions and ideas and his keen appreciation of new ideas expressed by other pupils. In the case of several others who made high scores, they are what one might call "shut-ins"—who for one reason or another have taken most of their play and recreation through imagination."

Three weeks of this procedure got us nowhere, because the setting was too complex and it was impossible to score the results. We then had the subjects make "one unified object" out of the 15 dots. Again it proved too complex for analysis, and eliminated creative design. We were also baffled with qualitative distinctions. There was only one thing left to do if we were to retain this method of approaching the problem, and that was to reduce the number of dots. Accordingly we took 6 dots and instructed the subjects to "make as many different drawings of designs or objects as possible by adding two dots to the six already given." The time was reduced from 45 to 15 minutes. This did not prove entirely satisfactory, because the six dots were too suggestive of definite contours of familiar common objects. To eliminate most of this we finally adopted the four-dot square as offering least suggestion and giving greater control in scoring. By giving the subject the freedom of placing the two "extra" dots anywhere he liked, and permitting him to use any kind of lines in making the drawings, ample opportunity was offered for the expression of his creative ingenuity.

The four-dot method is more tangible than a mere free association test, because it slows up the thinking process and gives more direct conscious attention to the evolving of a new idea. The pictured presentation of an idea gives us better ground for scientific investigation. We attempted to use the free association test by having the subject write as many words as possible within 15 minutes and then go through the list searching for the number of distinctly new ideas as they appeared. We had the subject go over his own list and state just where the breaks came in his thinking process while writing the list. Again we were swamped with complexities. There are certain possibilities, however, in this method. It is most difficult to ascertain the creative breaks in a list of free associated words.

All these preliminary experiments were given to four psychologically trained students in the department of Psychology in the Harvard Laboratory. One of the subjects was an artist and another was an instructor of Psychology in the Department. The other two were Seniors in Radcliffe College.

The artist proved to be most creative. She averaged 25.6 in the number of figures produced, and 22.8 in the number of "Creative Changes." The averages in the number of "Creative Changes" for the other three subjects were 15, 11.5, and 5, respectively. The number of figures drawn in 15 min. by these four subjects at their first trial with the four-dot method was 26, 18, 16, and 8. This gave an average of 17—being 1.9 lower than the average for the grade-school children given above. The number of "Creative Changes" for these four subjects at their first trial was 22, 15, 9, and 7, respectively. The average

of their "Creative Changes" was 13.2—being 4.75 greater than the average Creative Imagination of the 407 school-children. We make comparisons with the first trials only, because this eliminates any facility in performance gained through the subsequent trials. After four weeks' practice of one 15 min. period once each week, allowing the subject to repeat each week if he desired, we found that the averages were raised only about .5 in each case. This increase was almost constant in each subject.

VIII. Conclusion

To those who fail to recognize the value of this test as a method for the measurement of "Creative Imagination," we would merely suggest that possibly they could utilize the method to advantage in studying pure imagination. Our work thus far has been a play with method rather than with the solution of the problem. We believe, however, that our method is fundamental.

By joining a creative test such as we have outlined, with a "reproductive" test such as any general intelligence test, we shall get a more accurate statement of the worth of an individual. The intelligence test alone does not evaluate this vital "Creative Energy."

The test we have outlined deals primarily with a visual imagery stimulus to creative action. Probably some people would prove more creative in responding to an auditory stimulus. Some will remind us that this type of experiment does not draw out the potential "logical" creative capacities of an individual. We hold that it does. What could be more empty than four dots without any logical significance? It is true that we get an image of some object that we desire to draw, but the whole thinking process is involved in forming this image or association of neurograms. Visual imagery generally expands into scraps of kinaesthesis, auditory imagery and personal, organic or verbal references. Logical creative performance involves imagery. All imagery involves logical creative capacities. In the April issue of the American Journal of Psychology (1921), in an article by Claire Comstock on "Relevance of Imagery to Processes of Thought," we find the conclusion that "there is no irrelevant imagery."

This test gives equal chance of expression to the "dreamer of dreams" and the planner, the moulder and the constructor. The stream of imagination always flows in a determinate direction. One may think at first that a mere visual imagery process is of more value in finding artists than in finding lawyers. We must not fail to recognize, however, the transition which this "image" must undergo in being given symbolical representation in these drawings. The constructive mental mechanism of free association is supplemented by motor habit.

A TABLE FOR THE GRAPHIC CHECK OF THE METHOD OF CONSTANT STIMULI

By L. B. Hoisington

The graphic representation of the actual and of the theoret ical values of p in connection with the Method of Constant Stimuli may serve as much more than a mere rough check upon the accuracy of the mathematical operations. The relation of the actual to the theoretical distribution of percentages, when presented graphically, reveals to the student just coming into psychophysical work the results of his rather arduous labors in a way which he can grasp. To tell him that the steepness of the curve determines the value of h, and that the 50% ordinate where it cuts the curve gives L in terms of the abscissa units, may mean more or less to him,—usually the latter. If he plots the curve of actual distribution alone his case is but little better; he must plot the theoretically best fitting curve over the actual. His knowledge of the use of the principle of least squares to get the best fitting curve is no more complete than before, but he can see that it does come out of his labors. With the two curves before him it is much easier to make him understand the principle of the law of error and the relation of a distribution due to 'sampling' to a distribution based upon an infinite series. It is also an easy way to show the relation of the ogive curve to the 'normal' curve of distribution, a relation which is by no means as evident to the beginner without mathematical background of the right kind as one is likely to assume. It serves as an excellent opportunity to show why the value of L is different from any one of the stimulus-values used.

For these reasons and also because it does serve as a check, rather rough to be sure, upon the work of a student, the instructor in a drill course often does well to plot the theoretical curve. Boring[1] gives directions for deriving the values and plotting the curve as well as the pedagogical advantages which come from its use. Also, as Boring points out[2], if we are to apply the principle of the sum of the squares of the differences as a test of adequacy, we must at least find the values for p_t and

[1]E. G. Boring, Urban's Tables and the Method of Constant Stimuli, *Amer. Jour. Psychol.*, 28, 1917, 288 and 291.

[2]*Op. cit.*, 288.

γ	p	γ	p	γ	p	γ	p
.00	.5000	.50	.7603	1.00	.9214	1.50	.9832
.01	.5057	.51	.7646	1.01	.9234	1.51	.9837
.02	.5113	.52	.7690	1.02	.9254	1.52	.9842
.03	.5169	.53	.7733	1.03	.9274	1.53	.9848
.04	.5226	.54	.7775	1.04	.9293	1.54	.9853
.05	.5282	.55	.7817	1.05	.9312	1.55	.9858
.06	.5338	.56	.7858	1.06	.9331	1.56	.9863
.07	.5395	.57	.7899	1.07	.9349	1.57	.9868
.08	.5451	.58	.7940	1.08	.9367	1.58	.9873
.09	.5508	.59	.7980	1.09	.9384	1.59	.9878
.10	.5563	.60	.8020	1.10	.9401	1.60	.9882
.11	.5618	.61	.8059	1.11	.9418	1.62	.9890
.12	.5674	.62	.8097	1.12	.9434	1.64	.9898
.13	.5730	.63	.8135	1.13	.9450	1.66	.9906
.14	.5785	.64	.8173	1.14	.9466	1.68	.9913
.15	.5840	.65	.8210	1.15	.9481	1.70	.9919
.16	.5895	.66	.8247	1.16	.9496	1.72	.9925
.17	.5950	.67	.8283	1.17	.9510	1.74	.9931
.18	.6005	.68	.8319	1.18	.9524	1.76	.9936
.19	.6059	.69	.8354	1.19	.9538	1.78	.9941
.20	.6114	.70	.8389	1.20	.9552	1.80	.9946
.21	.6168	.71	.8424	1.21	.9565	1.82	.9950
.22	.6222	.72	.8457	1.22	.9578	1.84	.9954
.23	.6275	.73	.8491	1.23	.9591	1.86	.9958
.24	.6329	.74	.8524	1.24	.9603	1.88	.9961
.25	.6382	.75	.8556	1.25	.9615	1.90	.9964
.26	.6435	.76	.8588	1.26	.9626	1.92	.9967
.27	.6487	.77	.8619	1.27	.9638	1.94	.9970
.28	.6540	.78	.8650	1.28	.9649	1.96	.9972
.29	.6592	.79	.8681	1.29	.9660	1.98	.9975
.30	.6643	.80	.8711	1.30	.9670	2.00	.9977
.31	.6695	.81	.8740	1.31	.9681	2.05	.9982
.32	.6746	.82	.8769	1.32	.9691	2.10	.9985
.33	.6797	.83	.8798	1.33	.9700	2.15	.9988
.34	.6847	.84	.8826	1.34	.9710	2.20	.9991
.35	.6897	.85	.8854	1.35	.9719	2.25	.9993
.36	.6947	.86	.8881	1.36	.9728	2.30	.9995
.37	.6996	.87	.8907	1.37	.9737	2.35	.9996
.38	.7045	.88	.8934	1.38	.9745		
.39	.7094	.89	.8959	1.39	.9754		
.40	.7142	.90	.8985	1.40	.9762	2.40	.9997
.41	.7190	.91	.9010	1.41	.9770	2.45	.9998
.42	.7238	.92	.9034	1.42	.9777	2.50	.9998
.43	.7285	.93	.9058	1.43	.9785	2.55	.9999
.44	.7331	.94	.9082	1.44	.9792	2.60	.9999
.45	.7378	.95	.9105	1.45	.9799	2.65	.9999
.46	.7424	.96	.9127	1.46	.9806	2.70	.9999
.47	.7469	.97	.9150	1.47	.9812	2.80	1.0000
.48	.7514	.98	.9171	1.48	.9819		
.49	.7559	.99	.9193	1.49	.9825		

take the difference between these and the actual values of p. The appended table is not sufficiently accurate for this purpose except in a demonstrational way, if indeed a demonstration may ever be less refined than what careful work demands.

In order to make the change from γ_t to p_t easier and quicker Dr. J. M. Gleason, as mentioned by Boring[3], computed from the table of Bruns the values of p_t for all the two-place values of γ up to 1.50 and from there on every tenth value. It is this table, slightly extended, which we here present in the hope that it may be of use to others, in drill-courses if not otherwise. For very accurate work one must still go to the more extended tables of Bruns and Kämpfe.[4] Our table is, in intent, in line with other tables, such as Rich's checking table,[5] the purpose of which is to reduce the mere mechanics of the work to the lowest possible limit consistent with the degree of accuracy which the task in hand demands.

---

[3] *Ibid.*, 292.

[4] H. Bruns, *Wahrscheinlichkeitsrechnung und Kollecktivmasslehre*, 1906. B. Kämpfe, *Philos. Stud.*, 9, 1893, 147 ff.

[5] G. J. Rich, A Checking Table for the Method of Constant Stimuli, *Amer. Jour. Psychol.*, 29, 1918, 120-121.

ARE THERE ANY SENSATIONS?

By Robert M. Ogden, Cornell University

Are we not ready to dispense with the element of sensation in systematic psychology? Several lines of argument suggested by the present trend of psychological investigation lead to such a conclusion. The first of these is modern phenomenology. The second is the attack which has been made upon conscious entities by the behaviorists; and the third is the newer views of neurology, especially as they have been formulated by Henry Head and his associates.

I. The high points in the history of psychological experimentation, in so far as it has dealt with the nature of consciousness, have been, successively, the analysis of sensation, the investigation of memory, the attempt to bring the higher thought-processes under experimental control, and, lastly, the phenomenology of perception. In his account of the last Congress for Experimental Psychology which met at Marburg in April, Hans Henning tells us that a new psychology is now being born. "Until the turn of the century," he writes, "it was believed that one could grasp the mind with number and measure. This was the direction of the Wundtian School; however, few papers of this sort were to be found on the Congress programme; for since 1900 there has developed a qualitative psychology which concerns itself less with numbers and more with kinds of experience and qualitative analysis. One knows to-day that the complications and structure of experience can not be analyzed into simple qualitative elements and thus built up by joining one to another, but that one must work constantly with psychical forms (*Gestalten*)."

It is the investigation of these formulated structures that has led to a conception of psychological integration,[1] the elemental constituents of which are not psychical entities but aspects of an attributive order.

Even the earlier introspectionists were sometimes loth to admit that consciousness, when examined in cross-section, could be analyzed into so many entities, joined like the parts in a jig-saw puzzle. James, for instance, in his *Principles of Psychology*,[2] maintained that the taste of lemonade is some-

[1] See footnote next page.

[2] II, 2.

thing fundamentally different from sweet added to sourness, coldness, aroma, etc. The doctrine of mental chemistry, advanced to cover such synthetic discrepancies, was never entirely satisfying, because analogies of this sort are always inadequate to the factual data, and likewise for the reason that the chemical analysis of a compound into its elements is, indeed, exhaustive. The characteristics of water are the characteristics of its constituent parts, hydrogen and oxygen, and both these gases are susceptible of isolation and investigation. In the case of perception, however, analysis is more baffling; for the sensations therein contained reveal "aspects" such as extensity, intensity, and quality; and the entities or "contents" of which the perception is presumed to be made up can not be readily and individually isolated.

Although the logic of analysis is the same whether one analyse a material substance or a mental object, the constituents in the two cases are and remain distinct; and if it be the same objects of experience that give rise alike to the inferences of chemistry and the inferences of psychology, the inferences themselves are vastly different. One can carry over from one science to another the logical method and procedure, but never the precise point of view nor the existential data and technique of observation.

Perhaps we may regard the existential datum of all experience as a complex, having many facets, which, when viewed from diverse angles, reveals for each a unique phenomenological unit. It then becomes the problem of any special science to record the phenomena occurring under its view of the whole, and to infer the underlying order which constitutes the unity peculiar to its angle of observation.

Such an attempt was made in the older analyses of the conscious cross-section into sensations, images, and feelings. But the prejudice of quantification was too strong upon these earlier analysts; and their efforts were unsuccessful because an ex-

[1]The use of the term *integration* is derived from the writings of H. J. Watt; *cf.* Some Problems of Sensory Integration, *Brit. Jr. of Psychol.*, 1910, 3, 323ff. "An intimacy of connexion between *nerve-paths* or impulses emanating from different sense organs is, of course, recognized in many forms. But this connexion has been somewhat exclusively considered to consist in a *mere coordination* or association of afferent and efferent impulses with one another. Sufficient attention has hardly been paid to the possibility that upon these afferent impulses an afferent structure might be raised which is dependent upon but essentially an addition to these. To distinguish it from mere coordination such a structure might well be called integration" (323 f). See also "The Elements of Experience and their Integration: or Modalism," *Brit. Jr.*, 1911, 4, 127 ff.; and "The Main Principles of Sensory Integration," *ibid.*, 1912, 6, 239ff., from which the following sentence is taken: "So the word integration may imply the general theory of the relation of a mode (*Gestalt*) to its basis in experience, which psychology may hope some day to attain" (247).

haustive procedure of weighing and measuring was obviously impossible. The mental object or phenomenon which furnished the point of departure was always existentially complex. However simple it might be rendered under the conditions of experimental control, it still persisted in remaining an object with a certain self-constituted 'thingness' about it. The greater its simplicity, the more definitely it might be described; yet the absolute simplicity of a single mental entity, to be taken up and examined apart from every other entity of mind, always eluded one's grasp. Were its color to be observed, the color must also have shape and size, or at least area; it must possess a certain brightness of intensity and a certain duration. Even a slight variation in one of these aspects might be sufficient to alter the entity so profoundly that it at once became something else.

As Titchener has pointed out in his paper on *Sensation and System*, "the sensation of classification is the logical resultant of many observations. Its qualitative attributes are (in the typical case) points selected by definite procedure from a continuum; its intensive attributes are themselves continua, reduced by the same procedure to a series of points. Hence, the conjunction of the qualitative attributes with any points whatsoever upon the correlated intensive scales constitutes, for classification, 'a' sensation; the bracketing together of the qualitative attribute with the complete intensive scales constitutes 'the' sensation. It would be a great simplification of psychology if a sensation, *tota, teres, atque rotunda*, would stand before us under a single comprehensive determination and allow us to observe it as a whole. But that, if it ever happens, happens only after we have made many separate observations of its distinguishable aspects."[3]

The question raised by my paper is one of doubt as to the genuineness of the hypothetical elements of sensation. Even since Titchener wrote in 1915 the investigation of the psychological nature of perception has revealed some of the characteristic integrations of attributes which constitute the fundamental patterns of attentional awareness. If a perception in its bare phenomenal essence is not found to be an aggregate of sensory elements, but an integrated unit, which upon analysis is reduced to a number of attributive aspects rather than to a number of individual conscious particles, it would appear that the elemental sensation can be experienced only under a determination of perception; and that, in so far as it can be perceived at all, it is a percept in the same way that 'movement' or 'wetness' or 'the taste of lemonade' is a percept. As Rahn states the case, "sensory consciousness is no simpler than, but equally

[3]This JOURNAL, 26, 1915, 261.

as complex as, any other type of consciousness, and the laws of its coming and going are the same as those for the rise of any other form."[4]

To be sure, the perceptual patterns from which we derive our awareness of objects are not all of equal simplicity. In the different modalities of sensation, different types of integration are possible. One might assume for the purpose of defining a sensory element that it should consist in the integration of those attributes that characterize the modality under consideration whenever each enters into the unit once and once only. Thus a simple tone or element of sound would have one pitch-brightness, one volume, one intensity, and one duration, whereas a tonal fusion would be a more complex integration in which at least two attributes of pitch-brightness are involved. But there is still no evidence that this hypothetical entity ever appears in consciousness as a bare sensation, or that it would possess any special significance, apart from its logical simplicity, if and when it did. Indeed, within limits, the attributes of intensity and duration are relatively unimportant as compared with those of pitch-brightness and volume. The bare *quale* of the tone or of the fusion may be equally simple as psychological datum prior to the analysis which reveals the conditions under which the two are aroused.

But even if we accept this logical definition, may it not lead us into further difficulties? Not only is the elementary sensation to be marked off from those units of experience, like fusions, in which one attribute is used twice over; it must also be set off from other units in which the full quota of attributes belonging to a given modality is not involved in the integration. Thus visual movement as defined by Dimmick[5] is but *duration* and *quality*. *Extent* and *intensity*, which also enter into the definition of a visual sensation, are not essential to movement. Of course, the other attributes characteristic of the sense-modality might be 'found' if one were to search them out, but this would be another problem than that of observing the phenomenon of movement. If it be argued that in the first case we are analyzing the phenomena of a mental 'function,' and in the other the phenomena of a mental 'structure,' are we doing more than piling up logical distinctions? We might say that the 'functional' pattern of movement concerns some of the attributes, but not others; that the 'structural' pattern of a mental element concerns each attribute used once over; and that the more elaborate patterns of fusions and complications involve attributes used twice over, and may extend beyond the modality of one department of sense. Yet in each of these cases the unit of sensory

[4]Cf. Sensation in Contemporary Psychology, *Psychological Review Monogr. Series*, No. 67, 1913, 107.

[5]*This Journal*, 31, 1920, 317 ff.

experience remains an abstraction of certain attributes which integrate to form a conscious *quale*, and this *quale*, under an appropriate attitude of observation, is as genuine an element of experience as any other. It therefore appears that the primary integration of attributes which determines the original units of sensory experience may embrace either more or less than the full complement which has been logically assumed to define the sensation.

II. The movement known as behaviorism falls outside the scheme of development which has accepted the analysis of consciousness as its chief task. It is nevertheless a logical consequence of the earlier efforts to measure mind, and the ease with which many psychologists have accepted the principles of behaviorism seems to indicate an inherent difficulty in the conception of sensation by the readiness with which the stimulus is accepted in its place. The radical programme of the behaviorist, which denies the scientific validity of all contents of consciousness, has contributed its part toward weakening the position of the conscious element by the destructive criticism leveled against the older notions of mental processes. However, in confining its investigations to stimulus and response, behaviorism has raised new problems which it must be prepared to face if the restricted means with which it proposes to operate in its investigations are to prove adequate in the solution of its problems.

Two points occur to one in which the means commonly employed by the behaviorist seem deficient in the attainment of his ends, the one being his failure to control the mediation between stimulus and response, while the other arises from inexact knowledge as to the nature of the stimulus itself. The dependence of the integrative patterns of perception, and likewise those of ideation, upon definite conditions of arousal from without and equally definite conditions and states of neural, muscular, and glandular receptivity and response from within, may, however, furnish that understanding of the mediation between stimulus and response without which we are so often at a loss to predict the behavior of an organism even under objective conditions that have been well defined and carefully controlled. As Boring has pointed out in his recent paper on the *stimulus-error*, "if only the end-terms of stimulus and response are controlled, a universal one-to-one correlation between stimulus and response is not possible"; and he goes on to say that the "only way out of the dark would be to study the effect of stimulation, of attention, and of criterion by taking hold of these dependent series at their intermediate points, thus providing ourselves with a more complete knowledge and control of the entire psycho-physical situation."[6]

[6] This JOURNAL, 1921, 32, 449 ff.

The attempt to attain this "complete knowledge and control of the entire psychophysical situation" through study of psychological integration—rather than by assuming such artifacts as sensations and images, bound together by the chains of association—seems promising also with regard to the exact nature of the stimulating situation. In abandoning mental elements in favor of attributes of experience we are able to deal exclusively with psychological continua having a definite context in the objective conditions of stimulation and likewise in the apparatus and mechanism of response. For this reason the psychology of integration should prove a more serviceable instrument to the behaviorist than the psychology of mental elements which he now so cheerfully forswears. At any rate the student of behavior must be ready to fractionate his stimulus and study the influences upon his subject's response exerted by varying intensities, extensities, durations and the alteration and combination of sensory modalities. In so doing he will necessarily follow the procedure of the psychologist who continues to find his problem in the observation of attributive aspects and in the conditions that govern their integration.

III. With respect to Head's newer views of neurology[7] I speak with some diffidence; but I think it worthy of note that a distinction may and perhaps must be drawn between the sensory impulse and the resultant sensation. A parallel is suggested also between the psychological integration of which we have been speaking and the physiological integration of Head's theory.

Realizing that Head's results are still a subject of controversy, and that my own equipment as a neurologist is too meagre to give them adequate evaluation, I shall not attempt to seek any large measure of support for my thesis in this quarter. Yet when Head tells us that "psychological analysis fails entirely to disclose the struggle of sensory impulses revealed by (his) experiments",[8] we are at least challenged to consider his conclusions. In brief, Head maintains that "integration occurs as impulses pass from the periphery toward the higher centres; the change (being) a constant one from a complex to a simpler and more specific grouping," so that "sensation, the final end of the process, assumes forms simpler than any sensory impulses."[9] Again from this angle of approach we are invited to question the validity of sensation as we have been wont to conceive it. The precise sensory mechanism put forward by Head may or may not be acceptable, but upon superficial inspection it seems to be quite as plausible as the contrary view

[7] Cf. *Studies in Neurology*, London, 1920.
[8] *Op. cit.*, 329.
[9] *L. c.*

that an impulse started in a specific end-organ travels unchanged to the highest receptive centre where it supplies the neural correlate of the sensory element of experience.

Still another point is suggested by Head's reference of affection to the optic thalamus. The anomaly of feeling as an element in consciousness has been frequently discussed; for feeling lacks the objectivity that belongs to the hypothetical elements of sensation and image. Yet the acceptance of *affection* as an attribute has been equally unsatisfactory, because it is not an invariable accompaniment of the elements, otherwise supposed to be fully described in terms of their attributes. If, however, we shift our mode of attack, and regard affection as a phenomenon of psychological integration, we may find it no more remarkable that we should be affected by certain integrations than that the integrations themselves should be compulsory in provoking distinctive psychological units of experience. Perhaps, too, the problem of the image would be better solved if we were to give up the attempt to discover its textural difference from sensation and limit the investigation to its phenomenological make-up as an integration of attributes taking place under certain definite conditions of experience.

In the programme of research here proposed we may still take general classes of sensation as a point of departure, though we should no longer be striving to reconstruct mental life as a certain number of conscious entities merely joined one to another in a mosaic of sensory particles. Rightly considered, even this analogy is more adequate to the facts of experience than was originally intended; for the mosaic has a pattern which the artist follows in the selection of his pieces of stone. Likewise, in the living organism, the pattern of the whole dominates the accretion of minor parts. To resolve the whole into its parts without regard to their relatedness in the original is to violate the principle of analysis. We do not go to the mosaic to study the nature of stones, but to study the nature of design. When the whole is broken up into its constituent pieces there is no design left. To attempt to understand the nature of mind by breaking it up into hypothetical elements is still more futile, because the elements themselves either disappear, or reconstitute themselves in a new type of experience which is a different subject of analysis from the one with which we started.

But if we take the perceptual pattern as our basis of analysis and study its attributive aspects under conditions which we can control, it may be that the varied integrations of these attributes will reveal to us the fundamental patterns underlying all experience. We shall be dealing with the only elements of mind that are capable of treatment in isolation one from another, and at the same time in intimate dependence upon physical

conditions that can be exactly measured. Thus there seems hope for a common meeting ground between the investigator who by temperament and scientific interest insists upon limiting his field of research to the tangible data of the physical universe, and the investigator who finds his problem restricted to the phenomena of consciousness. While a reconciliation is hardly to be expected, or even perhaps to be desired, it is nevertheless of interest to the cause of science that all should work toward a common end; for surely no gain can arise from a flat refusal to admit the possible justification of another point of view than one's own.

THE INFLUENCE OF CREATIVE DESIRE UPON THE ARGUMENT FOR IMMORTALITY

By Cavendish Moxon, M.A.

Human curiosity first serves the self-centered impulse to seek objects which cause pleasure and to avoid objects which threaten pain. In proportion to the growth of ethical repressions of crude impulse and cultural sublimations of infantile desire, curiosity has been diverted to problems in the external world. Civilization has involved a severe limitation of the open play of fantasy and the direct satisfaction of desire. Only by means of elaborate religious, artistic, and scientific disguises can the primitive desires escape. Indeed the aim of scientists is to discover fact even when fact contradicts the deepest desire.

Since many precious wishes have been hidden below consciousness, it is clear that the psychological study of the unconscious inevitably arouses great affective opposition. This is an obvious factor in some of the adverse criticisms of Freudian theories. In some cases the unconscious opposition is strong enough to prevent the severe renunciation of pleasure involved in accepting the new hypotheses. In other cases the admission of unwelcome facts has to struggle against the rationalizations and moralizations of primitive impulse and self-centered desire. The struggle is strongly marked in the artistic temperament which delights in new symbolic representations of unconscious desires and in imaginary association of pre-conscious impressions. The creative artist has a strong will to live which is symbolized by belief in survival of death. Moreover the artist tends to be conscious of the poverty of his expressions in comparison with the wealth of his impressions. He will therefore be almost overwhelmed by the smallness of his productions when he learns that all his previous experiences have been impressed and preserved in his subconscious mind.

Such, it would appear, is the feeling of M. Maeterlinck, who has recently published his views about the preservation of impressions in what he terms the subconscious mind. The poet accepts the facts which he uses as a new way of rationalizing the belief in survival which satisfies his deepest desires. The weight of these desires may be estimated by their power to

obscure the poet's critical judgment on matters of fact. M. Maeterlinck's reason for belief in survival is similar to the argument based on the premisses that justice is supreme and that an enormous amount of conscious ability is destroyed by death before it is able to do its good work for the world. M. Maeterlinck modifies this argument by putting his emphasis on the waste of unconscious mental energy, and by substituting natural economy for divine justice. The poet as psychologist knows that the conscious waste of material represents but a small part of the vast mass of unused subconscious material in even the longest human life.

M. Maeterlinck has recently argued that man's unconscious soul survives his conscious and bodily death. His argument appears to rest on two premisses—a fact about the extent of man's wasted unconscious life, and a belief about the nature of organic life in general. The fact is proved by a very large number of observations made by hypnotists and psychoanalysts: and it is this. A countless number of sensations are preserved at subconscious levels of the mind, either in temporal succession or in more elaborately associated groups. The subconscious part of the psyche is therefore enormously larger than the conscious organ of perception and memory. From the immense and ever increasing subconscious accumulation there rise to consciousness only a few fragments, as memories, fantasies, jokes, automatisms, intuitions, or symbolic images. M. Maeterlinck stands in wonder before the store of psychic material revealed by recent research. With his vivid imagination the poet pictures the boundless possibilities for mental creation that are involved. His aesthetic conscience refuses to contemplate the final loss of so great a hidden treasure. Even the most productive genius cannot use every impression that has sunk into forgetfulness for lack of the appropriate stimulus for its recollection and use. Therefore, argues M. Maeterlinck, the retention of so many impressions is a useless mental process under the conditions of bodily life on earth.

At this point the poet seems to have been carried away by his emotional reaction to the new knowledge, and to have made an exaggerated statement about the uselessness of subconscious impressions. Certainly not every impression returns through the normal memory of even the oldest and busiest thinker. But the majority of subconscious impressions are at any time available for conscious use if required. The only exceptions are a few unpleasant or disgusting impressions which have been so deeply pressed into the unconscious that neither they nor their associated impressions can be recalled by memory. The unemployment of such undesirable images, however, is surely no loss which calls for a compensatory opportunity in a future

life. Of the vast majority of impressed experiences that lie within the limits of conscious recall, it is possible that any one may become a valuable memory in order to serve some individual or social purpose—to save a life, to compose a poem, to make a joke, to construct a theory or a machine. Any selective process in the subconscious might fail to include and preserve ideas or images that could be used again to preserve or enrich the personal and the collective life. Nature is a bountiful provider. No expenditure is extravagant if only it is directly or indirectly a means to the preservation and transmission of life. Millions of seeds are produced in order that a small minority may survive. It is better that countless germs should perish than that one should be lacking when required to subserve the instinct for the preservation of the species. Likewise it is worth M. Maeterlinck's keeping a subconscious packed full of idle words and images if, by a stroke of genius, some few may rise to conscious life as the elements of a poem or a play.

We are now in a position to estimate the validity of M. Maeterlinck's second premiss, which he asserts as if it were an undisputed scientific law: "It is admitted that Nature does nothing that is useless." This unqualified negative proposition is certainly not admitted by the present writer. There spring to his mind pictures of many monstrous products of Nature—useless additional limbs, innate morbid impulsive tendencies and instinctive disharmonies which have no value for the organism or the species as a whole. No doubt such entirely useless structures and modes of behavior are exceptions. The majority of organs and functions in Nature have at one time some degree of usefulness to the species. The appendix was not always a dangerous relic. And the tendency of organisms to produce a sudden variation of form or instinct usually promotes survival though it occasionally makes a self-destroying monster.

M. Maeterlinck's argument would be stronger if he could show that all the subconscious impressions are useless for earthly life. But he knows the loss to be only partial—thanks to memory and creative intuition. Now if many seeds are wasted in order that a few may be used, the natural inference of a mind unbiassed by unconscious desire would be that a host of subconscious impressions may be held in readiness to form a useful thought or a beautiful deed. M. Maeterlinck's premiss is so inaccurate as to invalidate his argument from the supposed uselessness of the subconscious impressions that a strictly utilitarian nature must provide another sphere of usefulness for the unconscious after death. Moreover, the usefulness of a subconscious impression is not wholly dependent upon its regaining consciousness. The psychoanalytical study of behavior has

made it probable that subconscious impressions when they become the images of desire indirectly play an important determining part in every act of judgment, choice, creation, and appreciation. The capability for attention, no doubt, strictly limits the recall of images; but M. Bergson showed forgetfulness to be useful for practical life. If memory were complete, choice and action would have an impossible task.

The biologist might be satisfied to have shown the weakness of M. Maeterlinck's argument. The psychologist's interest is by no means exhausted till he has gone a step further and explained the use of so weak an argument by so strong a mind. M. Maeterlinck is indeed only one instance of the paradox that eminent men often give illogical reasons for their belief in immortality. We must therefore generalise our enquiry and ask what it is that leads even men of genius to overlook the inadequacy of their reasons for this particular belief. The answer is given by the psychoanalysts, who have proved that men's reasons for belief in survival are unconsciously influenced by the desires they imperfectly conceal. The basic motives for the refusal to contemplate the annihilation of the ego are neither logical nor moral. The fact is, as Dr. Ernest Jones declares, that "in the unconscious every one believes in the omnipotence of his thoughts, in the irresistibility of his charms and in the immortality of his soul." The unconscious is not concerned with moral and metaphysical reasons for survival. The unconscious feels the primitive will to live. Death has no meaning for this level of the psyche, which refuses to think of the extinction of itself and of the objects to whom its love and interest have been transferred. In the unconscious dream-thoughts men appear to be alive many years after their death; time is abolished and the ego only "dies" or disappears in order to be reborn. The conscious arguments are therefore after-thoughts or rationalizations of the primitive wishes. The great unconscious weight of lowly psycho-biological desires accounts for the acceptance of many an argument, that, without this support, would be spurned by all intelligent men. Likewise the will to believe in survival without proof or *quia impossibile* is due to the unbounded egoism of the unconscious mind. Religious faith in the eternal moral values of the soul has its roots in the supreme worth of the unconscious ego in its own estimation; and belief in immortal life is a barrier against the fearful thought of wasted powers—powers which seem immense and are checked and limited by an indifferent world. A rationalistic psychology has supposed that the original cause of belief in immortality was the false reasoning of primitive man who believed he saw his dead friends alive in the world of his dreams. The new psychology, which seeks for wishes beneath thoughts, declares that

the savage saw his dead living in dreams because he could not conceive the intolerably unpleasant thought of the annihilation of those who ministered to the pleasure of his beloved and immortal self. The non-existence of time for the unconscious mind is proved by the fact that for many years after their death some parents continue to exercise a repressive and harmful influence over their family who only consciously accept the fact of their freedom, and show their bondage and fear in the dream-products of their unconscious life. An expression of the adult's unconscious refusal to face the fact of death sometimes openly appears upon the lips of the child. The following conversation reported by Dr. Jung makes this clear.

Anna, aged three, asks: "Grandmamma, why have you such faded eyes?"
Grandmamma: "Because I am old now."
Anna: "But that means that you will be young again."
Grandmamma: "No, I shall get older and older, you know, and then I shall die."
Anna: "Yes, and then?"
Grandmamma: "Then I shall be an angel."
Anna: "And then will you become a little child again?"

In night-dreams and to some extent in day-dreams is realized the desire of the self-centered psyche for a free and endless exercise of its powers in a perfect world. In metaphysical opinions and religious hopes the unconscious wishes have to compromise with the scientific interest in external reality. In men of poetical imagination like M. Maeterlinck, we see the fight of self-love for dominance, and the consequent imperfect rationalization of unconscious desire. M. Maeterlinck, we conclude, is led to use bad reasons in support of his belief in immortality because this belief is a necessary symbol of his unconscious desires, which dominate the thoughts in his conscious creed.

A BIBLIOGRAPHY OF THE SCIENTIFIC WRITINGS OF WILHELM WUNDT

By E. B. TITCHENER and S. FELDMAN

(*Seventh Supplementary List*[1])

1913

(6) *Die Psychologie im Kampf ums Dasein.* Zweite Auflage. Large 8vo. Leipzig. A. Kröner. Pp. iii., 38.

1914

(5) *Ueber den wahrhaften Krieg.* Rede. Small 8vo. Leipzig, A. Kröner. Pp. 40.
(6) *Reden und Aufsätze.* Zweite Auflage. Large 8vo. Leipzig, A Kröner. Pp. vii., 397.
(7) *Grundriss der Psychologie.* Zwölfte Auflage. With 23 illustrations. Large 8vo. Leipzig, A. Kröner. Pp. xvi., 414.

1915

(1) *Deutschland im Lichte des neutralen und des feindlichen Auslandes.* Scientia, xvii., 71-85. Also in French form as *L'Allemagne aux yeux des nations neutres ou ennemies*, Scientia, xvii., Supplément, 59-72.
(2) *Die Nationen und ihre Philosophie.* Ein Kapitel zum Weltkrieg. Erste und zweite Auflage. Large 8vo. Leipzig, A. Kröner. Pp. vii., 146.
(3) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. vi. Mythus und Religion, dritter Teil. Zweite, neubearbeitete Auflage. Large 8vo. Leipzig, A. Kröner. Pp. xii., 564.
(4) *Karl Lamprecht.* Ein Gedenkblatt. [Von Wilhelm Wundt und Max Klinger.] With a portrait. Lex. 8vo. Leipzig, S. Hirzel. Pp. 18.

1916

(1) *Die Nationen und ihre Philosophie.* Ein Kapitel zum Weltkrieg. Kröners Taschenausgabe. Small 8vo. Leipzig, A. Kröner. Pp. 154.
(2) *Leibniz.* Zu seinem zweihundertjährigen Todestag. Large 8vo. Leipzig, A. Kröner. Pp. v., 132.
(3) *Elements of Folk Psychology: Outlines of a Psychological History of the Development of Mankind.* Authorized translation by E. L. Schaub. London, George Allen & Unwin, Ltd.; New York, The Macmillan Co. Pp. xxiii., 532.

1917

(1) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vols. vii., viii. Die Gesellschaft. Large 8vo. Leipzig, A. Kröner. Pp. viii., 438; vi., 434.

[1]The first installment of this Bibliography appeared in the JOURNAL for October, 1908. Supplementary lists were published in the following October numbers up to and including the year 1914.

1918

(1) *Die Nationen und ihre Philosophie.* Ein Kapitel zum Weltkrieg. Kröners Taschenausgabe. 16th-25th thousand. Small 8vo. Leipzig, A. Kröner. Pp. 154.

(2) *Einführung in die Psychologie.* Vierter Abdruck. 19th-23d thousand. (Ordentliche Veröffentlichungen der pädagogischen Literaturgesellschaft "Neue Bahnen.") 8vo. Leipzig, Dürrsche Buchhandlung. Pp. vi., 123.

(3) *Grundriss der Psychologie.* Dreizehnte Auflage. With 23 illustrations. Large 8vo. Leipzig, A. Kröner. Pp. xvi., 414.

(4) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. ix. Das Recht. Large 8vo. Leipzig, A. Kröner. Pp. xviii., 484.

(5) *Das Reformationsfest.* Beiträge von Protestanten und Katholiken: Avenarius, Bonus, Eucken, Geyer, Gogarten, Hoffmann, Mumbauer, Natorp, Planck, Roethe, Rosegger, Tönnies, Troeltsch, Weinel, Wundt. Flugschrift 173 des Dürerbundes. Large 8vo. München, G. D. W. Callwey. Pp. 49.

1919

(1) *Vorlesungen über die Menschen- und Tierseele.* Sechste, neubearbeitete Auflage. With 53 illustrations. Large 8vo. Leipzig, L. Voss. Pp. xvi., 579.

(2) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. iii. Die Kunst. Dritte, neubearbeitete Auflage. With 62 illustrations. Large 8vo. Leipzig, A. Kröner. Pp. xii., 624.

(3) *System der Philosophie.* Vierte, umgearbeitete Auflage. 2 vols. Large 8vo. Leipzig, A. Kröner. Pp. xvi., 436; vi., 304.

(4) *Logik.* Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. i. Allgemeine Logik und Erkenntnistheorie. Vierte, neubearbeitete Auflage. With illustrations. Lex. 8vo. Stuttgart, F. Enke. Pp. xvi., 654.

1920

(1) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. x. Kultur und Geschichte. Large 8vo. Leipzig, A. Kröner. Pp. ix., 478.

(2) *Die Zukunft der Kultur.* Schlusskapitel aus Band x. der *Völkerpsychologie.* 8vo. Leipzig, A. Kröner. Pp. 54.

(3) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. iv. Mythus und Religion, erster Teil. Dritte Auflage. With 8 illustrations. Large 8vo. Leipzig, A. Kröner. Pp. xii., 587.

(4) *Die Weltkatastrophe und die deutsche Philosophie.* Beiträge zur Philosophie des deutschen Idealismus. (Veröffentlichungen der deutschen philosophischen Gesellschaft, herausgegeben von A. Hoffmann. Folge der Beihefte.) Sechster Heft. 8vo. Erfurt, Keysersche Buchhandlung. Pp. 16.

(5) *Erlebtes und Erkanntes.* Large 8vo. Stuttgart, A. Kröner. Pp. xii., 399.

(6) *Einleitung in die Philosophie.* Achte Auflage. Mit einem Anhang, etc. Large 8vo. Stuttgart, A. Kröner. Pp. xviii. 448.

(7) *Grundriss der Psychologie.* Vierzehnte Auflage. With 23 illustrations. Large 8vo. Stuttgart, A. Kröner. Pp. xvi. 414.

(8) *Logik.* Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. ii. Logik der exakten Wissenschaften. Vierte, neubearbeitete Auflage. With illustrations. Lex. 8vo. Stuttgart, F. Enke. Pp. xv. 671.

1921

(1) *Logik.* Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. iii. Logik der Geisteswissenschaften. Vierte, umgearbeitete Auflage. Lex. 8vo. Stuttgart, F. Enke. Pp. xii., 693.
(2) *Kleine Schriften.* Vol. iii. Large 8vo. Stuttgart, A. Kröner. Pp. vi., 549.
(3) *Probleme der Völkerpsychologie.* Zweite, vermehrte Auflage. Large 8vo. Stuttgart, A. Kröner. Pp. vi., 217.
(4) *Elements of Folk Psychology: Outlines of a Psychological History of the Development of Mankind.* Authorized translation by E. L. Schaub. [Revised edition.] London, George Allen & Unwin, Ltd.; New York, The Macmillan Co. Pp. xxiii., 532.
(5) *Die Nationen und ihre Philosophie. Ein Kapitel zum Weltkrieg.* Kröners Taschenausgabe. 26th-30th thousand. Small 8vo. Stuttgart, A. Kröner. Pp. 154.
(6) *Erlebtes und Erkanntes.* Zweite Auflage. Large 8vo. Stuttgart, A. Kröner. Pp. xii., 399.
(7) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. i. Die Sprache, erster Teil. Vierte Auflage. With 40 illustrations. Large 8vo. Stuttgart, A. Kröner. Pp. xv., 695.
(8) *Metaphysik.* In Die Kultur der Gegenwart, etc. Vol. i., Abt. 6. Herausgegeben von P. Hinneberg. Dritte durchgesehene Auflage. Lex. 8vo. Leipzig, B. G. Teubner. Pp. x., 408.

NOTE.—The following publications have been transferred from W. Engelmann to A. Kröner:

1874 (2) *Ueber die Aufgabe der Philosophie in der Gegenwart.*
1876 (2) *Ueber den Einfluss der Philosophie auf die Erfahrungswissenschaften.*
1879 (1) *Der Spiritismus.*
1887 (6) *Zur Moral der literarischen Kritik.*
1901 (2) *Sprachgeschichte und Sprachpsychologie.*
1906 (4) *Essays.*
1907 (7) *Outlines of Psychology.*
1907 (11) *System der Philosophie.*
1910 (7) and 1911 (4) *Kleine Schriften*, vols. i., ii.
1911 (6) *Hypnotismus und Suggestion.*
1911 (7) *Naturwissenschaft und Psychologie.*
Grundzüge der physiologischen Psychologie. Sixth and last edition. 3 vols.
Völkerpsychologie. All current editions.

Magazines.—Wundt's *Psychologische Studien* ended with vol. x., whose title-page is dated 1917, though its final *Heft* appeared in 1918. The last three parts (Hefte 3, 4-5 and 6) receive the subtitles Königlich Sächsische Forschungsinstitute in Leipzig: Forschungsinstitut fur Psychologie, No. 1, No. 2, No. 3.

Wundt continued his consulting editorship of the *Archiv f. d. ges. Psychologie* to the time of his death in 1920 (vol. xl.).

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LVIII. An Experimental Investigation of the Experience Which Accompanies the Sudden Cessation of an Auditory Stimulus

By Elisabeth W. Amen

In his experiments upon the positive after-image in audition Bishop[1] found that a physically sudden cutting off of his stimulus-tones was not accompanied by a sharply ending tone for sensation. The transition from full tone through the changed character of the auditory experience to the ultimate disappearance of all sound of the tone he called the "modified ending." Throughout his experiments he tried to eliminate this modified ending, but was never successful. He concluded, at the end of his research, that the modified ending might be "due in part to tonal *Abklingen*, in larger part to the objective conditions" of his experimental arrangement.[2]

Problem

It is the purpose of this study to continue the investigation of the modified ending. Not every *O* had reported the modified ending at every stimulation. Only a few experiments were made by Bishop in which his *Os* were instructed to listen for it at every stimulation. These few led him to believe that it was always present, but the evidence supporting this belief was confined to the data obtained from a few experiments made at the end of his work. The fitful manner in which the modified ending made its way into the reports of his *Os* was the source of so much annoyance, that he sought relief from its irregularity of appearance in the possibility that it was really always present, and that his instructions to the *Os* had diverted them from it, so that they reported it only when it was sufficiently striking to force itself upon them. Our own first task, then, was to extend the number of experiments made under the instruction to listen for the modified ending at every stimulation, and thus to discover, if possible, what its existential character might be.

Experimental

Bishop's results indicated that the apparatus used in Series II[3] of his experiments could be utilized for our experiments. In that form it was simpler and more easily kept in adjustment than in some of his later forms. The only change from his arrangement in our first group of experiments was the omission of the sound-proof box. We decided that this would not be needed, because any echo would follow the final portion of the modified

[1] H. G. Bishop, "An Experimental Investigation of the Positive After-Image in Audition," this Journal, 32, 1921, 305-325.

[2] *Ibid.*, 325.

[3] *Ibid.*, 310.

ending and would, consequently, not interfere with the observation. With only the summer term of six weeks at our disposal we confined ourselves at the start to one pitch, 512 vs, at the three intensities used by Bishop.[4]

Observers. The *Os* were Assistant Professor Josephine Gleason of Vassar College, and Assistant Professors K. M. Dallenbach and L. B. Hoisington, Dr. H. G. Bishop, and Miss C. C. Braddock (graduate student in psychology), of Cornell University. All *Os* were experienced, but Bi was more practised in observation of the modified endings than the others.

Instructions: "You will hear a tone which will be cut off suddenly. About two seconds before it is cut off the signal flag will fall as a warning. In attributive terms, describe any change in the tone which occurs as it is going off."

Series I

In this series Bi, Br, D, and G were given 30 stimuli in haphazard order, 10 at each one of the three intensities. We found that echo, particularly at the higher intensities of stimulation, was a distraction, and that the stimulus-tone must therefore be enclosed in a sound-proof box. Every *O* except G, who unfortunately described the echo, reported some kind of transitional ending for every tone. This regularity was highly satisfactory; but the lack of uniformity in the descriptions of the nature of the modified ending was less comforting. The characteristics of the modified ending which were commented upon referred to its pitch, intensity, and volume, and to phases of it which are similar to certain phonetic sounds like labials and aspirates, such as "plop," "whoop," or "breathiness." Besides these there were less frequent reports that the modified ending was "noisier," "less singing," "less brilliant," "more blunt" than the tone. It was a "rustling" to which no pitch could be assigned, it "thinned," and was "hollower" than the tone. We give below a summary of the results for every *O*. S, M, and W refer to the three intensities, and the numbers designate the number of reports covering the characteristic of the "modified ending" to which *O* referred.

PITCH

Bi: S, flatting 8; M, flatting 5, sharping 3; W, flatting 2, sharping 3
Br: S, sharping 2; M, flatting 1, sharping 4; W, flatting 2, sharping 1
D: S, sharping 6; M, flatting 1, sharping 4; W, flatting 0, sharping 3

INTENSITY

Bi: S, less 3; M, no reference; W, greater 1
Br: S, greater 2, less 1; M, less 2; W, greater 1, less 4
D: S, greater 1, less 6; M, less 7; W, greater 0, less 4

VOLUME

Bi: no reference
Br: S, less 1; M, no reference; W, no reference
D: S, less 7; M, less 4; W, less 3

DURATION

The modified ending is practically instantaneous, and most reports do not refer to its length. Occasionally, however, a very short or an unusually long ending is mentioned.

---

[4] *Ibid.*, 307.

This summary was made by counting the number of recurrences of any report by any *O*, and took no account of the fact that the stimulus-tones, presented to all *O*s at the same time, furnished another measure of the degree of agreement among *O*s. We found, upon investigation, that there was about the same amount of contradiction among *O*s whether we compared the reports as they are given in the summary or whether we compared the reports of all *O*s for the same tone.

Observation of the modified ending is very difficult; indeed, it is so difficult that we trusted to practice and the elimination of echo to bring the uniformity of report which we desired. Without further change than the boxing-up of the variator we proceeded with Series II.

Series II

In this series 60 stimuli were presented in haphazard order, 20 for every intensity. The same contradictions which we found in Series I appeared again; but practice worked a change in the kind of report, which altered the significance of the contradictions. In the beginning the modified ending was baffling to every *O*. It is very short; and, since it begins with a tone, there was strong predisposition to regard it as tonal and to assign tonal attributes to it. Our instructions favored this kind of report, though they did not exclude references to other observational characters which the modified endings might show. Reference to these other characters came earlier in the reports of some *O*s than in others, but the course of change within the reports of all *O*s was similar. They began, as we have indicated, with references to pitch, intensity, vocal character, and more infrequently to volume. Soon they began to mistrust their observations of pitch and said "doubtful rise" or "doubtful fall." Another development was the increasing observable complexity of the ending. It was not unitary but complex, and had a well-marked temporal course in spite of its brevity. It began with tone and ended with something more noisy, and during its course it seemed that the tone died away as the noise increased. This shift was expressed sometimes as a vocal change like oo-oop, and sometimes as an "aspirate" character or a "breathiness." Sometimes there was said to be a change in relative intensity or relative volume between the two phases of the ending. Everyone heard a decided change in the ending of every tone, but to give a verbal account of it severely tested every *O*'s descriptive and observational ability. The results showed further that the different *O*s attended to different aspects of the ending, and this fact produced heterogeneity of report even when there was the unifying factor of simultaneous observation of the stimulus-tones. The descriptions were so varied that unification into a typical modified ending for every intensity was well-nigh impossible. Every intensity gave the ending, and the stronger the intensity the more complex and more striking was the ending. Since it was our task to reduce the variability of the descriptive accounts, we made a second shift in technique for Series III, in which we used the weak stimulus only, on account of its simpler ending.

Series III

In spite of variability in what the *O*s said about the nature of the modified ending (57 stimuli in this series), there remains the more pertinent fact that some kind of transitional ending to the tones was always found. We desired first to secure evidence that tones were never cut off sharply for sensation, and secondly to find out what we could about the observable character of the ending. Throughout our study we have always found that the tones showed definite transformation as they died away.

Even though we did not find identity and full agreement in the descriptions, their very differences can be turned to good account. The

differences indicated that the *O*s either were not picking out the same aspects of the modified ending for report, or were observing under different degrees of proficiency due to different degrees of practice. To make a descriptive composite picture of the modified ending from the data at our disposal may be presumptuous; but we hazard such a picture, for it should be harmless, if it is regarded as a tentative construction, and should be useful as a stimulus to further experimentation.

There is a temporal course to the modified ending which begins with the first noticeable change in the character of the tone and continues until complete silence has set in. The verbal account will suggest that the modified ending is relatively long; but such a conception is highly erroneous; for its duration is of the same order as that of the click of a telegraph sounder. The pitch, which was clear and ringing during the tone, suddenly is encroached upon by a new quality which is said to be "breathy," "noisy," "hissy," etc. These two exist together, as flowing experiences, not static. The tonal part diminishes and the noisy part increases. The original tonal character to which the pitch was assigned very soon disappears. It is impossible, from the reports, to say whether it dies without change, whether it grows more shrill, as if it were present at a higher degree of attributive clearness, or whether it perhaps undergoes some other change. At this stage the experience is both tonal and noisy, and it is difficult to decide whether the tone-noise compound has a new pitch, or whether it is nonsense to speak of pitch at all. All agree that this stage is qualitatively different from the pure tone; and it is certain that the flatting or sharping reported is not the pitch-change of the musical slur. Following this middle stage, the tonal character diminishes to zero and the noise component reaches its maximum. The course of the noise begins with the breathiness which has already been mentioned and progresses into something hissy resembling the sound of *s* in *this*. It is very much softer and more breathy than *s* but is similar to it. The *s* passes over into a very soft, non-explosive, aspirated *p* similar to *p* in *up*, especially to that part of the *p* which is made by closing the lips as the breath comes against them; but not to the explosive part which accompanies the opening of the lips and the release of the compressed air in the mouth cavity. The modified ending could not be imitated accurately by any *O* in speech; but phonetic elements were easily recognizable in it, even if there was not full agreement about the letter which best expressed them. It is somewhat uncertain whether the tone continues until the end of the modified ending; some reports say that it does, others that it does not. In the matter of intensity there is also uncertainty. It seems probable that the general intensity of the modified ending is less than the intensity of the tone, although the weakening in the tonal component may perhaps be compensated by an increasing intensity of the noisy component. As regards the vowel quality of the tone, there is fairly general agreement that it is the phonetic *o*, or an admixture of another vowel with *o*. The clear vowel is, however, clouded by an admixture of the noise already described. Naturally, attributive volume has no significance if the pitch is constant. Reports on pure attributive volume were made much more infrequently than on other characters, if they were ever made. We are of the opinion that the coloring of noise rendered it impossible to judge pure tonal volume.

This, then, completes the picture which expresses the best founded generalizations from the results of Series III. It had to be pieced together from reports which were not the result of simultaneous attention of the *O*s to the same aspect of the tones. We hoped, therefore, to obtain greater uniformity by requesting them to attend in one set of observations to pitch, in another to vocality, and in a third to intensity. We devoted an hour's observation to each one, and finished the experimentation by a return for an hour to the instructions of Series III.

Series IV

Instructions: "Pick out the most striking pitch in the tone, during its course, and listen to it to see if it remains the same in the modified ending, or if it changes. If it changes, report the direction of change. If the pitch drops out before the end of the modified ending, tell what it is that follows."

The effect of this instruction was a gain only in this, that the reports made were on pitch. G said the pitch dropped; Br found that it sometimes rose, sometimes fell, sometimes remained the same; for D it generally dropped; for Bi it remained the same until it vanished. All alike reported an occasional lag of something breathy or aspirate after the tone had ceased. Since Bi is the most practised *O*, and since he too, during the earlier observations, reported a change in pitch, we suggest that more practice for the other *O*s might bring them to the same conclusion. H, who is a highly practised *O*, in the few observations which he made judged that the pitch dropped out, and that in so far as he could assign pitch to the non-tonal or less tonal aspirate part of the modified ending it flatted at the very end. This report is partial agreement with Bi, at any rate; the reports of Bi in some cases say that there is a different character at the end of the modified ending, which seems to be of lower pitch than the tone.

Instructions: "In this set of experiments you are to give your attention to the vowel quality. If there is a perceptible transition in the vowel quality from the ending of the tone to the ending of the modified ending, express this transition in phonetic characters; and describe it more fully, if possible, in whatever additional terms you choose."

Except for G, who said there was no change in vowel quality, every *O* reported a transition. For some it began with almost pure *o*, for others with about equal purity of *u*; it finished with something resembling explosiveness expressed by *p*, *h*, or *t*. Br inserted *e* and *i* during its course, but no other *O* indicated intermediate stages.

Instructions: "In this set of experiments you are to give your attention to intensity. Report the direction of any perceptible change in it, and add whatever additional description you can."

Again there was disagreement. The different *O*s did not agree either with themselves or with one another. All reported increase, decrease, and the same intensity. We suggest that the irregularity may be due in part to a confusion of intensity and attributive clearness. Bi reported "shrillness" which he never defined; but it seemed to resemble either a rise in intensity or an increase in clearness.

Return to Original Instructions. The added practice and the successive direction of attention to pitch, vowel quality, and intensity had a unifying effect upon the reports of pitch. Every *O* except D said that the pitch dropped out rather than changed; D reported, in several cases, that it was the same, with one report that it rose. In other respects the data taken in this last hour were typical of what has been said in the generalized description of Series III.

Conclusions. (1) We always found the modified ending of Bishop's experiments.

(2) The modified ending is certainly qualitatively different from the stimulus-tone; but we cannot be positive about changes in other respects.

LIX. The After-effect of Movement in the Sense of Touch

By Wellington A. Thalman

In his study "On the After-Effect of Seen Movement" Wohlgemuth reported a series of 34 experiments, the last one of which was conducted to see whether an after-effect of movement existed in the sense of touch similar to that in the sense of sight.[1] The result was negative, his conclusion being that "under the given experimental conditions, no analogous after-effect of movement exists in the case of touch."[2] In the theoretical sections which follow the experimental, this conclusion was further qualified: "the negative result of this experiment must not be considered as final; some observations made since then show me that the subject is worthy of further investigation."[3]

The present paper reports a series of experiments, which were undertaken in the interests of this problem during the Summer Term of 1921.

Observers.—The *O*s were Miss Catherine Braddock (*B*), fellow in psychology; Dr. Josephine Gleason (*G*), assistant professor of psychology at Vassar College; Dr. Karl M. Dallenbach (*D*), and the author (*T*). When the author observed, Miss Elizabeth Amen, graduate student in psychology, acted as *E*. All the *O*s were trained in introspection. They observed an hour a day, five days a week, and, in so far as it was possible, at the same hour every day. *B* and *G* worked without knowledge of the problem.

Experiments 1 *and* 2

We first sought to reproduce Wohlgemuth's experiments. He gives such a meager account of them, however, that instead of referring to his work we shall give a complete description of the method and procedure which we used.

Method and Procedure—A string-belt of cotton wrapping cord,[4] knotted at intervals of 4 cm., was driven over two horizontal drums at three rates of speed: slow, medium, and fast; moving respectively 7, 14, and 36 cm. per sec.[5] The moving stimulus was applied to the under side of the bare fore-arm, which was smoothly shaved so as to eliminate the drag and pull of the cord on the hairs. The fore-arms were alternately used, in order to exclude, as far as possible, the effects of fatigue and the after-images of pressure; consequently the direction of movement, which objectively was constant from the *O*s' left to right, was alternately ulnar-radial and radial-ulnar. The arm was placed between the two drums, directly over and at right angles to the lower warp of the belt. The hand grasped a support, and, at a given signal from *E*, *O* lowered his elbow to a padded rest, thus bringing his arm into contact with the moving stimulus. The hand-grasp insured the constancy of the place stimulated. To avoid fatigue the position of the hand-grasp, and consequently the position of stimulation, was changed between every two experiments with the same

[1] *Brit. Journ. of Psych.*, Mon. Suppl. 1, 1911, 88, 109.

[2] *Op. cit.*, 88.

[3] *Op. cit.*, 109.

[4] We first tried a silk thread, the "thin silk cord" of Wohlgemuth, but abandoned it as the silk cut the skin and aroused complicating sensations of pain. In its place we used cotton twine, which, being larger and softer, did not have the saw-like effect of the other cord.

[5] Wohlgemuth does not state what speeds he used. He merely says that "various rates" were employed. We do not know, therefore, whether we duplicated his experiments in this respect.

arm. The areas stimulated were restricted to positions between 5 and 20 cm. from the first carpal folds. Three stimulation times, of 60, 120, 180 sec., were used.[6]

The apparatus was so arranged that the movement could be brought to an abrupt end, either with the cord in contact with the arm, or with it forced away from the arm.

In the first series of experiments the cord was forced away; in the second, the cord was allowed to remain in contact with the arm. In each series every stimulation time was used with every rate of movement and the whole repeated 5 times, making a total of 45 experiments for every *O*. The experiments were conducted in haphazard order.

Directions.—The directions used in these experiments were: "At the 'Ready' signal place your arm in position. At 'Now', close your eyes and lower your elbow to the padded rest. A continuous moving stimulus will be applied to your arm. When the objective movement has ceased and the resulting phenomena have run their course, give a complete account of them." These instructions brought out such a great mass of extraneous data regarding the sensations and perceptions aroused during the objective stimulation that the following was added. "Just before the objective movement is stopped *E* will give a second 'Ready, Now' signal. Give particular attention in your report to the phenomena, if any, which occur after the objective movement ceases."

Results.—Nothing was said in the instructions about after-images of movement; indeed, as we observed above, two of the *O*s worked without knowledge of the problem. We expected, nevertheless, since the *O*s' attention was directed to the interval immediately following the objective movement, that reports of negative after-effects would be given if such phenomena occurred. And such phenomena did occur. All *O*s reported instances in which the objective movement was followed by a movement in the opposite direction. The percent. of times these negative after-images of movement were reported is shown in Tables I and II. The results of the first series of experiments, those in which the cord was removed from the arm, are shown in Table I; the results of the second series, those in which the cord was allowed to remain in contact with the arm, are shown in Table II.

TABLE I

Showing the percent. of times that an after-effect of movement was observed when the cord was removed from contact.

	RATE	SLOW			MEDIUM			FAST		
	Time in sec.:	60	120	180	60	120	180	60	120	180
	B	20	40	40	20	60	40	0	0	20
	D	0	0	0	0	0	0	0	0	0
O	G	0	0	0	0	0	0	0	0	0
	T	0	0	0	0	0	0	0	0	20

TABLE II

Showing the percent. of times that an after-effect of movement was observed when the cord was allowed to remain in contact.

	RATE	SLOW			MEDIUM			FAST		
	Time in sec.:	60	120	180	60	120	180	60	120	180
	B	40	40	0	40	60	40	20	20	40
	D	20	0	0	20	20	0	0	0	20
O	G	20	0	0	0	20	0	0	0	20
	T	0	0	0	0	20	20	0	20	20

[6]Again, Wohlgemuth is not explicit; he merely reports "the experiments lasted from 1-3 minutes." *Loc. cit.*

In the experiments of Series 1, only two *Os*, *B* and *T*, reported the after-effect; but in Series 2, all *Os* reported it. The experiences were variously described as a "jerk backwards," a "slow movement backwards," a "diffuse drift backwards," a " backward wave-like movement," a "reverse movement," a "backward movement," and a "backward brush or sweep." The rate, extent, and duration of the subjective movement varied considerably: it was described as rapid and as slow; as large and as small; as of long duration and as of short.

The after-effect was noted more frequently when the stimulus was allowed to remain in contact with the arm than when it was removed; more frequently when the medium rate of movement was used than when either the fast or the slow rate was employed; and more frequently when the stimulus was applied for 120 sec. than when it was applied for the longer or shorter intervals.

We recognize that the negative after-image of movement occurred too infrequently in our experience to permit of our drawing conclusions regarding the conditions of its appearance. Our only claim is that our results demonstrate the existence of an after-effect of movement in the sense of touch analogous to that in the sense of sight.

Compulsory conditions were not obtained for any of the *Os*. The closest approach was with *B*, who reported the negative after-effect in both series of experiments in 60% of the cases when the medium rate of movement and the 120 sec. stimulation were used. That we did not obtain compulsory conditions we believed to be due to the fact that only small areas of the skin, areas approximately 1 mm. x 30 mm. in extent,[7] were stimulated. It is true, as our results themselves demonstrate, and as Wohlgemuth clearly showed in the field of vision, that an after-effect may be obtained when the objectively moving stimulus is confined to very small areas; but there is nothing in our results, nor, as we read Wohlgemuth, is there anything in his results,[8] that would lead us to believe small areas are as compulsory as large. In the endeavor, therefore, to obtain compulsory conditions and to examine the effect of size upon the illusion, we increased, in the following experiments, the dimensions of the stimulus.

Experiments 3 *and* 4

Method and Procedure.—In experiments 3 and 4 the width of the stimulus was increased by replacing the string belt by a muslin band 12 cm. wide. The apparatus used in Experiments 1 and 2 was otherwise unchanged.

The method of procedure, however, was changed. (1) Five stimulation times of 10, 20, 30, 60 and 120 sec. were employed instead of three; and (2) the number of repetitions was reduced from five to three. The stimulation times were increased in number in order that a greater temporal variation might be obtained and the effect of duration be more easily observed. The extreme stimulation of 180 sec. was omitted because we found that it was not as effective as the 120 sec. interval. The number of times the various experiments were repeated was reduced to three because of pressure of time.

---

[7]The diameter of the cord was 1mm. The length of the area stimulated varied for the different *Os*; 10 successive measurements for every *O* gave the following: *B*, 32 mm. ± 3.1; *D*, 39 mm. ± 3.7; *G*, 31 mm. ± 3.0; *T*, 30 mm. ± 2.5.

[8]One of Wohlgemuth's experiments—the 23rd—was undertaken "for the purpose of examining Exner's statement that an after-effect is only produced by moving surfaces of some size" (*op. cit.*, 73). Wohlgemuth used surfaces as small as 0.075 mm. x 5 mm. and found that negative after-effects were still produced. He did not, however, quantify his data, or compare the after-effect produced by small areas with that produced by large.

In the experiments of Series 3 the cloth band was removed from the arm when the objective movement ceased; in those of Series 4 it was allowed to remain in contact with the arm.

Results.—All of the *Os*, in both series of experiments, reported the negative after-effect of movement. The same descriptive terms were used as before.

In the experiments of Series 3, those in which the belt was removed from the arm, a new after-effect was reported, which, like Benussi's loop and bow movements,[9] seemed to leave the skin. It was described in such terms as: "movement away," "movement off," "movement from." We do not know whether these experiences were the result of the sudden formation of a negative pressure gradient, or of a stimulus-error; it may suffice at present to note that they occurred, and that they were not classified with the negative after-images of movement with which we are now concerned.

There was little difference in the results between the two series of experiments, or among the various speeds of stimulation. The differences among the stimulation times, however, were quite marked: the 10 and 20 sec. stimulations gave very few negative after-effects,—none in fact for *D* and *G*,—whereas the longer stimulation times gave the after-effect in 33, 22, 26, and 32 percent. of the experiments for *B*, *D*, *G*, and *T* respectively. At these longer intervals, the cases occurred about equally often.

The negative after-effect of movement was reported more frequently in these experiments than in those of Series 1 and 2, but still compulsory conditions were not obtained. An increase of width alone was not sufficient; therefore, in the following experiments, the length of the area stimulated was also increased.

Experiments 5 and 6

Method and Procedure.—The apparatus in experiments 5 and 6 was adjusted so that the stimulus could be applied longitudinally. A rest, supporting the arm at the hand and elbow, was built over one of the drums at an angle of about 45°, so that the rotating belt could be raised or lowered by means of a movable table pivoted at the lower end of the rest, and contact with the arm could thus be made or broken. The belt, 12 cm. wide, made of the coarsest corduroy obtainable, was driven at the same three rates of speed used in the previous experiments. The shaved under surfaces of the forearms were alternately used. The direction of movement was peripheral, from elbow to wrist. Three stimulation times of 30, 60 and 120 sec. were employed. The length of the stimulated areas was approximately 18 cm., the width, that of the forearm. In the experiments of Series 5, the table was lowered and the belt was allowed to fall away from the arm; in those of Series 6, the movement was abruptly stopped and the belt allowed to remain in contact with the arm. In each series every stimulation time was used three times in haphazard order with every rate, thus making a total in each series of 27 experiements for every *O*. The same instructions were used as in the previous experiments.

Results.—As in the other experiments, all *Os* reported the negative after-image of movement. More cases were reported in these than in any of the previous series. Their distribution shows that the stimulus was more effective when it remained in contact with the skin after the interruption

[9] V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psych.*, 29, 1913, 385; Kinematohaptische Scheinbewegung und Auffassungsumformung, *Ber. ü. d. VI. Kong. f. exp. Psych.*, 1914, 31; Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psych.*, 36, 1916, 59.

of the objective movement than when it was removed; and furthermore, that there was little difference among the rates and the times of stimulation, —what little advantage existed was, however, in the direction of the faster rates and the longer times.

Though more reports of the after-effect were given in these experiments than in any of the previous ones we still failed to realize compulsory conditions. This failure we believed to be due to the inadequacy of the stimulation. The corduroy belt, on which we had set great hope, proved unsatisfactory. It was so soft and smooth, and the corrugations were so close together, that the *Os* frequently had difficulty in perceiving movement even during the objective stimulation; at times the movement adapted out and only a dull pressure remained; at other times, when the objective movement was perceived, the direction shifted.[10] It was not unusual for the *Os* to report that the direction had fluctuated 3 or 4 times during the course of a single stimulation. It was indeed surprising that so many reports of the after-effect were obtained under such poor conditions. In view of these facts, we decided to repeat the experiments with a more effective stimulus.

Experiments 7 and 8

Method and Procedure.—In experiments 7 and 8 a coarsely corrugated muslin cloth was used in place of the corduroy. The corrugations were made by sewing small pieces of cloth 2 cm. wide at separations of 4 cm. across the belt. The apparatus used in Experiments 5 and 6 was otherwise unchanged.

The method of procedure, however, was altered in two respects. (1) The direction of the movement was changed from peripheral to central; and (2) only two rates of stimulation were employed, which we shall designate, to bring them into line with the rates already used, as 'fast' and 'very fast'. The rate of movement during the 'fast' stimulation was 39 cm. per sec., approximately the 'fast' speed used in the other experiments; that during the 'very fast' stimulation was 109 cm. per sec., approximateiy three times the speed of the 'fast' rate. Otherwise the same procedure was followed as in Experiments 5 and 6. The instructions were not changed.

In the experiments of Series 7, the belt was allowed to fall away from the arm; in those of Series 8 the movement was stopped while the belt remained in contact with the arm.

Results.—The percent. of times that the negative after-effect of movement was reported in these experiments is shown in Tables III and IV.

TABLE III

Showing the percent. of times the after-effect was reported when the belt was allowed to fall from the arm, distributed according to the rate of the objective movement and the duration of the stimulation.

	RATE	FAST			VERY FAST		
	Time in sec.:	30	60	120	30	60	120
	B	100	100	100	33	33	33
O	G	33	33	67	100	33	67
	G	33	33	67	100	100	0
	T	0	0	0	67	33	100

[10]These experiences were analogous to those in the Bourdon illusion. *Cf.* B. Bourdon, *La perception visuelle de l'espace*, 1902, 176, 194.

TABLE IV

Showing the percent. of times the after-effect was reported when the belt was allowed to remain in contact with the arm, distributed according to the rate of the objective movement and the duration of the stimulation.

	RATE	FAST			VERY FAST		
	Time in sec.:	30	60	120	30	60	120
	B	67	67	100	100	67	67
	D	67	100	100	33	67	100
O	G	33	100	100	100	100	100
	T	0	0	67	33	100	100

These tables show that compulsory conditions were obtained for all *O*s. Certain combinations of rate and duration, more or less peculiar to every *O*, invariably produced the negative after-effect. Though the distribution of the cases seems to indicate a tendency for the phenomenon to occur at the faster rates and the longer durations, we recognize that too few experiments were performed to justify us in drawing any general conclusions regarding these variables.

We feel justified, however, in concluding, from the data at hand, that the conditions are more compelling for the perception when the stimulus is not removed from the arm; that, in other words, pressure stimulation continued after the cessation of the objective movement is conducive to the perception of the negative after-effect. This conclusion is warranted not only by the fact that compulsory conditions were obtained more frequently when the belt was allowed to remain in contact with the arm than when it was removed, as shown in Tables III and IV, but also by the fact that more cases of the after-effect were reported in the experiments of Series 8 than in those of Series 7. Indeed, this conclusion is corroborated by the results of all our experiments: more cases of the after-effect were reported, as is clearly shown in Table V, in the experiments of Series 2, 4, 6, and 8, *i. e.*, in those in which the stimulus was allowed to remain in contact with the arm after the objective movement had ceased, than in those of Series 1, 3, 5, and 7, the series in which the objective stimulus was removed.[12]

TABLE V

Showing the number of times the after-effect was reported in the different experiments: those in which the stimulus was removed, and those in which it was allowed to remain in contact with the arm.

REMOVED		CONTACT	
Exp.	Number times after-effect reported	Exp.	Number times after-effect reported
1	13	2	26
3	19	4	28
5	17	6	28
7	40	8	53
Total	89	Total	135

[12]A total of 1080 experiments was performed. Half were performed with the stimulus removed, and half, under otherwise identical conditions, with the stimulus continued in contact after the objective movement had ceased. When the stimulus was removed the after-effect was reported 89 times, in 16 percent. of the experiments; when contact was continued the after-effect was reported 135 times, in 25 percent. of the experiments. This difference is too large to be due to chance.

These results accord with those of Wohlgemuth in the field of vision. Wohlgemuth found "that it is far more difficult to discover the after-effect in the subjective field of vision than in the objective. In other words, the after-effect is more easily discovered if the field of vision is filled."[13] By 'subjective', Wohlgemuth means the field of vision when the objective movement is interrupted by closing the eyes,—a condition comparable to the removal of the cutaneous stimulus; by 'objective', he means the field when the movement has stopped and the eyes are open and focused upon the fixation point,—a condition comparable to the retention of the static cutaneous stimulus on the skin.

Experiment 9

In order to ascertain the effect of movement upon some other area of the body a few experiments were conducted upon the calf of *T*'s leg. This part of the body was chosen as it was the only other area that could be stimulated without radically altering the apparatus. Mr. W. A. Andrews, a graduate student in psychology, acted as *E* during these observations.

With the exception that the direction of the movement was peripheral, from the knee towards the foot, the apparatus was used exactly as in Experiments 7 and 8.

Results.—The results of the previous experiments were confirmed. Negative after-images were again reported. As far as *T* was able to discern, the after-effect on the leg was as pronounced as that upon the fore-arm. These results, therefore, lead us to believe that an after-effect of movement may be obtained in the sense of touch from any part of the body that is adequately stimulated.

Experiment 10

After we had obtained compulsory conditions, a few experiments were performed in which the *O*s were asked to describe the negative after-effects in strictly psychological terms.

The apparatus and method used in Experiments 7 and 8 were again employed.

The instructions were altered to read as follows: "In the previous experiments you reported negative after-images of movement. You are now to concentrate upon the description of these processes. At 'Ready, Now' the objective movement will cease; when the resulting phenomena have run their course, describe the processes in strictly psychological terms."

Results.—An analysis of the introspections yielded the following results.

(1) The negative after-images of movement were variously localized by the different *O*s. At times the *O*s reported that the movement was cutaneous, at times that it was subcutaneous, and at other times a combination of the two. Excerpts, selected by way of illustration, are:

(*B*) "Creeping along *surface.*"
"Movement uniform, smooth, weak, *subcutaneous.*"
"Movement *cutaneous as well as subcutaneous.*"
(*D*) "Light, filmy pressures which seemed to *envelop the arm* and float toward the wrist."
"Weak, light pressure, like puff of air moving on *surface of skin* with a duller, deader, *deeper*, more diffuse *pressure below the skin.*"
(*G*) "Even pressure moving *along arm.*"
"Flow of pressure *through* the arm."
"Bright, tingling pressure which moved *down the arm*, accompanied by a dull pressure which was *deeper* within the skin."

[13] *Op. cit.*, 31, 111.

(*T*) "Impression of movement floating *along* arm."
"Movement seemed to be *in, rather than along,* the arm."

(2) The quality of the after-effect varied with the localization. When localized in the cutaneous tissues, the after-images were described as "bright tickle," "light contact," "lively pressure;" when localized in the subcutaneous tissues, they were described as "vague," "dull," "dead," "deep," pressures. Examples are chosen from the reports of *D* and *G:*

(*D*) "After-effect of movement good..... Quality of the after-images *bright, lively pressure,* and *deep, dull, diffuse pressure.*"
"*Bright tickle* sensations which moved along the surface of the skin."
"*Light* superficial *contact* which seem to float along skin."
(*G*) "After the objective movement ceased at first perceived a *bright, lively pressure* which seemed to shift toward the wrist; then a *vague, dull pressure* localized deeper within the arm."

(3) The interval between the cessation of the objective stimulation and the appearance of the after-effect varied considerably in length. At times it was very short, and the negative after-images seemed immediately to occur:

(*D*) (The objective movement was) "*immediately* followed by a short jerk backwards."
(*T*) "Movement perceived *as soon as* objective movement ceased."

At other times it was longer, and the negative after-images were slow in appearing:

(*D*) "Light, filmy pressures which seemed to.....float toward the wrist, *slow* in appearing."
(*G*) "Dull pressures, seemed to shift toward wrist, came on more *gradually* than usual."

(4) The intensity, duration, rate, clearness and extent of the negative after-images likewise varied. At times the subjective movement was described as "intense" and "strong;" at other times as "weak," and "slight." At times it was reported to have "faded out very quickly" (*B*), to have been "of very short duration" (*D*), and to have "lasted just for a moment" (*G*); at other times it was reported to have "decreased slowly" (*B*), to have "long continued" (*D*), to have been of "long duration" (*G*), and to have "slowly faded" (*T*). The rate of the subjective movement was described as "rapid," "average or medium," and as "slow." At times the clearness was maximal, "as clear as if the cloth had been set going backwards" (*D*); at other times it was minimal: "the movement obscure, would not have observed it had I not been set for it" (*D*). The extent of the after-movement was at times definite: "the moving area was sharply and clearly defined" (*T*); but for the most part the extent was "diffuse," "ill defined," and "ill localized."

(5) The existential correlate of the perception of the negative after-image of movement appears to be an integration of spatial, intensive, and temporal aspects of the cutaneous or subcutaneous pressure sensations. The introspections upon which this statement is based are:

(*D*) "Slow backward movement along the surface of the arm. Bright lively pressure. The cutaneous sensations fluctuate spatially and temporally in intensity; by that I mean the cutaneous sensations, which are present over the whole area stimulated, rise and fall in intensity in a regular temporal and spatial sequence. The experience is clear, but very difficult to describe in attributive terms."
(*G*) "Dull pressure; movement gradual, seems to be spatial shift of intensity; quality did not change, it was the same dull pressure throughout."

" 'Running' on skin. The 'running' is a rapid spatial shift, changing in intensity."

Our analysis agrees very closely with that of Whitchurch given in her study on the illusory perception of cutaneous movement. Whitchurch found that the perception of movement obtained by stimulating adjacent pressure spots could be described as "an integration of quality, time, and cutaneous extent."[14] For methods which differ so widely, the descriptions are remarkably alike. If the difference is a true difference; if movement may in one instance be a qualitative integration with time and extent, and in another an intensive integration with time and extent; then we have further evidence that the meaning of movement "may be carried by several existential correlates."[15]

Summary

(1) Repetition of Wohlgemuth's work showed:
(*a*) that his apparatus was inadequate to the problem;
(*b*) that even under the unfavorable conditions of his experiment, all the *Os* reported an after-effect of movement in the sense of touch analogous to that in the sense of sight;
(*c*) that the after-effect was noted more frequently when the stimulus was allowed to remain in contact with the arm than when it was removed;
(*d*) that only a very small area of the arm was stimulated;
(*e*) that the apparatus must be modified, if compulsory conditions are to be realized, so that more effective stimuli can be applied.

(2) Experiments with the modified apparatus showed:
(*a*) that an increase of width alone is not sufficient to produce conditions compulsory to the after-effect;
(*b*) that an increase of width and length is still inadequate to the compulsory perception, if the stimulus is soft and smooth;
(*c*) that compulsory conditions are obtained when the fore-arm is longitudinally stimulated by a rough and coarsely corrugated linen band;
(*d*) that conditions are more compelling when the stimulus is not removed; that, in other words, pressure stimulation continued after the cessation of the objective movement is conducive to the perception of the negative after-effect.

(3) Experiments upon the calf of the leg showed:
(*a*) that the negative after-effect is as pronounced on the leg as upon the fore-arm;
(*b*) that the same general conditions obtained as upon the fore-arm.

(4) The experiments performed for the processual description of after-effect revealed:
(*a*) that the processes are at times cutaneous, at times subcutaneous, and at other times a combination of the two;
(*b*) that the quality varies with localization from "bright tickle," "light contact," "lively pressure," to "vague," "dull," "dead," "deep pressure;"
(*c*) that the interval between the cessation of the objective stimulation and the appearance of the after-effect is not constant; at times it is very short, at other times long;
(*d*) that the intensity, duration, rate, clearness and extent of the negative after-images vary; and
(*e*) that the after-effect is an integration of intensity, time, and cutaneous extent.

[14] A. K. Whitchurch, The Illusory Perception of Movement on the Skin, this JOURNAL, 32, 1921, 488.

[15] *Op. cit.*, 489.

LX. Haptical Illusions of Movement

By William A. Andrews

The present study was suggested by Benussi's results in his experiments on the tactual illusions of movement.[1] Benussi, thinking to duplicate in the field of touch Wertheimer's work in vision,[2] stimulated successively two spots on the skin separated by distances ranging from 4 to about 170 cm. (the distance between the finger-tips with outstretched arms) and by times ranging from 160 to 2200 σ. He obtained, among other kinds of movement, a "bow" movement, which his *Os* described as the movement of 'something' up from the skin through the air and down to the second point touched, "eine Bogenbewegung in der Luft."[3]

The object of the present investigation was two-fold: (1) to determine the optimal conditions under which this kind of movement appears; and (2) to describe the experiences in strictly psychological terms and to identify, if possible, the existential correlates of the 'something' which moved away from the skin.

Observers.—The *Os* were: Dr. J. M. Gleason (G), assistant professor of psychology at Vassar College; Dr. Karl M. Dallenbach (D); and Mr. W. A. Thalman (T), graduate student in psychology. G and D were highly practised in cutaneous observation; T, though trained in introspection, had not observed before in a cutaneous experiment. G and T worked without knowledge of the problem other than that given them in the instructions. All the *Os* observed an hour a day, six days a week, and so far as possible at the same hour every day. The experiments were performed during the Summer Session of 1921.

We first sought to obtain the 'bow-movement' and to determine the optimal conditions under which it appeared. Our idea was to secure these results in a series of preliminary experiments and then, using the optimal conditions, to turn in the main experiments to the problem of description.

Preliminary Experiments

In the preliminary experiments the following instructions were read to the *Os* at the beginning of every experimental hour: "At 'Ready, Now' your forearm will be stimulated. Characterize fully the cutaneous perceptions aroused, using any common-sense terms you wish."

Apparatus and Procedure.—In these experiments the volar surface of the left forearm for D and T and of the right for G was stimulated by two successive pressure points. The arm was smoothly shaved and held comfortably in a fixed position by a plaster cast. A modified form of Benussi's kinohapt[4] was used in applying the stimuli. In order that no distracting temperature sensations should be aroused, the pressure points were made of hard rubber. The diameter of the rounded tips was 1 mm. An area of approximately 0.8 sq. mm. was therefore stimulated whenever the points were applied to the skin.

The stimuli were successively applied along (or parallel to) the longitudinal axis of the arm. The second stimulus was always peripheral to

[1] V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psych.*, 29, 1913, 385; Kinematohaptische Scheinbewegung und Auffassungsumformung, *Ber. ü. d. VI. Kong. f. exp. Psych.*, 1914, 31; Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psych.*, 36, 1916, 59.

[2] M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych.*, 61, 1912, 161 ff.

[3] *Ber. ü. d. VI. Kong. f. exp. Psych.*, 1914, 32.

[4] *Arch. f. d. g. Psych.*, 29, 1913, 385 ff.

the first. Before every experiment the pressure points were carefully set by a fine adjustment to a distance of 0.5 mm. above the surface of the skin. The current operating the kinohapt, which was controlled by a shunted rheostat, was checked several times during an observational hour. The strength of the current was constant; only a few times during the entire experiment did the milliammeter show a variation. Since the excursion of the pressure points and the strength of the current were constant, the intensity of the stimulus may likewise be regarded as constant.

The kinohapt was controlled by a Leipzig time-sense apparatus, driven by a Ludwig-Baltzar kymograph. The duration of each stimulus was 150 σ. The temporal intervals between stimuli were 100, 600, 1100, and 1600 σ; and the spatial intervals were 2, 6, and 10 cm. Each stimulus was applied but once; that is, there was but one application of the bi-membral stimulus during a single experiment[1]. The experiments were performed in series of 10. Every temporal interval was used in haphazard order with every spatial interval. At the end of 12 groups, which this procedure necessitated, the experiments were continued by reversing the order; the 13th series was like the 12th, the 14th like the 11th, etc. The practice-effect was thus evenly distributed throughout the series. Two precautions were taken to guard against fatigue: (1) the pressure points were moved to new areas after every experiment; we were careful not to stimulate the same point twice during a single hour; and (2) at the end of every series *O* rested his arm a few minutes by taking it from the cast. Usually three series of 10 experiments were conducted during an experimental period. Since 20 reports were given for every combination of the variables, a total of 240 reports was obtained for every *O*.

Results.—G and T did not report movement phenomena of any kind. Their experiences were described as two completely independent impressions. They characterized the impressions, localized them, mentioned the temporal sequence, etc., but never reported movement, or anything that could be interpreted as movement. D, on the other hand, reported the phenomenon in about 45% of the experiments. His movement reports were of three kinds:

(1) *Unimembral.*—One member was perceived as moving; sometimes it was the first, and sometimes the second. When the movement was at the first it was always in the direction of the second; when it was localized at the second, two types were distinguished: (*a*) the movement was in a peripheral direction away from the first; and (*b*) the movement was in a central direction toward the first. Examples are: "Two touches, second peripheral to first. First one seems to move toward second." (100σ, 6 cm.) "First one seemed to be stationary and second seemed to fly off." (1600σ, 6 cm.) "Second seemed to jump backwards toward first." (600σ, 6 cm.)

(2) *Bimembral.*—Both members moved. Two types were reported: (*a*) both members moved from points of rest; and (*b*) the first moved from a point of rest, and the second to a point of rest. Examples are: "Two touches which seemed to rebound from the skin." (1600σ, 2 cm.) "Touch left skin and landed peripherally about 3 inches away, partial loop, not complete at top of arc." (1600σ, 2 cm.)

(3) *Full movement.*—The movement was complete from the first point to the second. Two types were described: (*a*) the first impression "hopped," "skipped," or "jumped" to a new position; and (*b*) the first impression, "slid," "glided," or "was dragged" to a new position. Cases of the

[1]This procedure we knew to be at variance with Benussi's; we adopted it, however, as we thought that the repetition of the stimulus would give rise, through suggestion, to subjective conditions. We were desirous in these experiments of restricting our investigation to objective conditions.

second type occurred infrequently; only 6 cases were reported during the preliminary experiments. Examples of these two types are: "First jumped to new position, complete arc." (100σ, 2 cm.) "Touch which glided on surface of skin to new position." (100σ, 2 cm.)

The reports in which no movement occurred were of two kinds. (*a*) The impressions, though discrete in time, were localized at a single spot, as: "Two touches, seems as if the same spot was touched twice." (1100σ, 6 cm.) (*b*) The impressions were discrete in space as well as in time, as for example: "Touched twice in very rapid succession, points discrete, second peripheral to first." (100σ, 6 cm.)

Since the objective conditions were constant throughout the experiments, and since D and G were about equally proficient in cutaneous observation, we believe that the difference between the reports of D and those of G and T, in regard to the perception of movement, is due to a difference of attitude. It is certain that the subjective conditions were not the same: G and T, as we have said before, worked without knowledge of the problem; they approached the experiments naively; whereas D, who was familiar with the problem and knew the object of the present research, was set for movement. This fact was clearly established by one of D's early reports, in which he said, after characterizing the "bow" movement: "I am attending to the stimulus, I am set for movement, and an effort is made to obtain it." Under this attitude, which we shall call the "meaning"attitude, the objective conditions were at times sufficient to give rise to perceptions of movement; but under the attitude which G and T assumed, the objective conditions were never sufficient to produce the perception.

Though the object of the preliminary experiments was thus defeated, —optimal conditions for a 'bow' movement were not obtained,—the results are significant. They show that objective conditions are alone not adequate to the perception of movement; that we are dealing with a perception which is in part dependent upon subjective conditions;[6] and that we must establish the "meaning" attitude if we are to parallel Benussi's results.

Two methods of establishing the proper subjective conditions immediately suggested themselves: the one, a direct method, was to give the suggestion openly in the instructions by telling the *Os* what we wished them to attend to; the other, an indirect method, was so to cut the objective conditions that the suggestion would come from these themselves. For obvious experimental reasons the latter method was chosen for the Main Experiments.

Main Experiments

Procedure.—The procedure was altered in but one respect: the bimembral stimulus was applied in every experiment a number of times in rapid succession.[7] The time-interval between every two pairs was twice the length of the interval between each member of the pair. That is, when the temporal interval between the members of a pair was 100σ, the temporal interval between the pairs was 200σ, etc. We thought that this procedure would strengthen the association between the members of a pair, and that it would through expectation and habituation lead to the suggestion of movement. In order that the effect of this multiple stimulation might be the better observed and the formation of the meaning attitude (if such an attitude were formed) might be the better studied, the pairs were

[6] This agrees in substance with the conclusions of Whitchurch (A. K. Whitchurch, The Illusory Perception of Movement on the Skin, this JOURNAL, 32, 1921, 486 f.).

[7] This is evidently the procedure that Benussi used, though he is not explicit,—he does not tell us, for example, how many times the pairs were applied or what time-intervals elapsed between the pairs.

applied 5, 10 and 15 times in succession. Every temporal and spatial interval mentioned in the preliminary experiments was used 5 times in haphazard order with every one of these multiple stimulations: thus, a total of 180 experiments was performed for every *O*.

Instructions.—The instructions were extended to include process as well as meaning. "Two points on your arm will be stimulated in rapid succession. When I say 'now' (which was said after 5, 10, or 15 repetitions) (*a*) characterize fully in any common-sense terms you wish the cutaneous perceptions aroused and (*b*) describe the perceptions in purely psychological terms." 'Process' instructions were added partly because we thought that they would indirectly, as if by contrast, assist in the establishment of the subjective conditions necessary to the realization of the movement perception, and partly because we still hoped to obtain the existential correlates of the illusory movement.

Results.—Our attempt to establish thus indirectly the necessary subjective conditions was on the whole successful. All the *O*s reported illusory perceptions of movement. D reported movement of some kind in approximately 80% of the experiments, G in 92 %, and T in approximately 22%.

In addition to the perceptions reported in the preliminary experiments, D reported four new types.

(1) A new type of bimembral movement was reported, in which each point moved a short way in the direction of the other, as for example: "Two touches, each seemed to jump part way toward the other." (1100σ, 10 cm., 10 rep.) (2), (3), and (4). Three new types of full movement were reported. In one the movement was backward, from the second to the first; in another the movement was alternately forward and backward; and in the third the movement was double, two arcs diverging from one central point to two peripheral points. Examples are: "..... Movement reversed and appeared to go backward, that is centrally." (600σ, 2 cm., 15 rep.) "Movement back and forth between the two points touched." (1600σ, 10 cm., 10 rep.) "Peripheral loop movement seemed to end at two different points." (1600σ, 2 cm., 15 rep.)

Though G reported movement phenomena in 92% of the experiments, she reported but two kinds of movements: unimembral movement, and full movement. Only two cases of unimembral movement were reported, and they were both of the same type; the first member moved in the direction of the second. These cases are: "Two touches, upper flicked off." (1600σ, 6 cm., 5 rep.) "Two successive touches, first flicked off in direction of the second." (1600σ, 6 cm., 5 rep.)

In 91% of the experiments G reported full movement. She distinguished four types. (1) The movement was from the first to the second; when of this type it took at times the form of an arc, bow or loop, as for example: "Pair of touches, hopping from point to point, always forward not backward." (100σ, 6 cm., 15 rep.). At other times it was on or in the skin as: "Movement peripheral, slid on skin to position of second touch." (100σ, 10 cm., 10 rep.)

(2) The movement was backward from the second to the first, as for example: "Direction of movement veered and went backwards. Backward movement does not come in until series has run for some time." (600σ, 2 cm., 15 rep.)

(3) The movement was alternately forward and backward, as: "Back and forth arc over arm touching it in two points." (600σ, 6 cm., 10 rep.)

(4) The movement was double, diverging in two arcs from a central point to two peripheral points. "Three touches, one central and two peripheral; meaning of movement from central to peripheral points in two diverging arcs." (1600σ, 2 cm., 10 rep.)

In the experiments in which no movement was reported, G characterized the experiences as "discrete temporally and spatially." In most

cases only two impressions were described, but in a few experiments three discrete points were reported, one central and the other two peripheral,—the static equivalent of the double movement mentioned above. "Three discrete touches, one central, two peripheral. One of the peripheral was like the central, the other was sharper. No movement of any kind." (1600σ, 2cm., 10 rep.)

T, like D, reported three kinds of movement: unimembral, bimembral and full movement.

(1) Five types of unimembral movement were reported; in two the movement was localized at the first member, and in three the movement was localized at the second. When it was localized at the first member it at times left the skin, as: "First point jumped toward the second. Very definite, about half an inch." (100σ, 6 cm., 15 rep.) At other times it seemed to be in or on the skin, as: "Spread or slid down arm in direction of second." (600σ, 10 cm., 10 rep.) When the movement was localized at the second member it at times left the skin, as: "Jumping from the second toward hand, movement very definite." (600σ, 10 cm., 10 rep.) At times it seemed to be in or on the skin, as: "Second point seemed to creep on." (100σ, 6 cm., 5 rep.) At other times it seemed to penetrate the skin and go into the tissues beneath, as: "Second point appeared to move toward a third point deep under the skin."

(2) Two cases of bimembral movement were reported, in which both members of the bimembral stimulus seemed to move, as: "Both points seemed to float off the skin." (1600σ, 10 cm., 5 rep.)

(3) Though but 6 cases of full movement were reported,[8] two types were characterized. In the first the movement was in the form of an arc from the first to the second member, as: "Succession of arcs moving from the first to the second." (1600σ, 10 cm., 10 rep.) In the other the movement was subcutaneous, as: "Subcutaneous movement which went successively from the first to the second." (1600σ, 10 cm., 10 rep.)

The most favorable conditions of the illusory perception of movement differ greatly for the several observers. The optimal temporal interval is 100σ for D, 1100σ for G, and 1600σ for T; the optimal spatial interval is 10 cm. for D, 6 cm. for G, and 10 cm. for T. The only variable moment that the *O*s agree upon is repetition; a greater percent. of movement phenomena is reported with 15 repetitions than with either 10 or 5 repetitions, and a greater percent. is reported with 10 repetitions than with 5.

The conditions most favorable to movement in general are also, for G and T, the conditions most favorable for the bow or loop movement,[9] but for D a different array is shown: the optimal temporal interval for bow movement is 600σ as opposed to 100σ; and the optimal spatial interval is 2 cm. as opposed to 10 cm. As before, however, 15 repetitions are the most compelling.

---

[8]In an effort to increase the number of bow movements, a supplementary experiment was conducted with T after the completion of the Main Experiments. The apparatus and procedure were the same as before, except that every temporal interval was used but once with every spatial interval, and that the stimuli were repeated 15 times in every experiment. The direct method of establishing the subjective conditions was used. We told T, in the following instructions, what we wished him to attend to: "Benussi obtained in his work upon the illusions of movement a loop or bow effect; that is, the first impression seemed to jump through the air to a new position, the position of the second stimulus. In the subsequent experiments, attend for this perception." Under these conditions T reported full bow movement in every experiment.

[9]This result would necessarily follow for G, since most of her reports were of the arc or bow type.

Compulsory conditions were not obtained for either D or T, but were variously obtained for G. No matter, within our limits, what temporal or spatial intervals separated the two stimuli, G reported movement in every experiment when the bimembral stimulus was repeated 15 times. G also reported movement in every experiment when the stimuli were repeated 10 times at the spatial interval of 2 cm. and the temporal interval of 1100σ, at the spatial interval of 6 cm. and all the temporal intervals, at the spatial interval of 10 cm. and the temporal intervals of 100, 1100, and 1600σ; when the stimuli were repeated 5 times at the spatial interval of 2 cm. and the temporal intervals of 100 and 600σ, at the spatial interval of 6 cm. and the temporal intervals of 600 and 1100σ, and at the spatial interval of 10 cm. and the temporal intervals of 100, 1100, and 1600σ.

The fact that there is so great a diversity in the results indicates that the perception of movement haptically aroused is not primarily dependent upon objective conditions. The essential requirements seem to be that the *Os* shall have the idea of movement and that this idea be given time for realisation. This conclusion is confirmed by the fact that the more frequently the stimuli are repeated, the more frequently are movement phenomena reported; and also by the fact that the *Os* in their introspective reports trace the gradual development of the perception from discrete points through unimembral and bimembral movement to complete or full movement. Examples are:

(D) "Bow is not complete at first, but with every successive stimulation it became more so until at the end it was complete." (15 rep.)

"At first bimembral movement. By concentrating on first member and by holding it in consciousness, complete arc movement resulted." (15 rep.)

"At start of experiment the impressions discrete; then first impression seemed to move, the second remaining static; then movement appeared at second; then movement was carried across in arc." (15 rep.)

"Character of movement changed during the course of the experiment. At first, first member moved; then second; then, forward arc movement; then backward arc, at which point the experiment ended." (15 rep.)

"At first bimembral movement. With successive stimulation the break or gap in the loop became smaller and smaller until just before the experiment ended the loop was completed." (15 rep.)

(G) "Discrete at first, then grouped in pairs, and then after a few stimulations got back and forth swinging notion of object touching arm at two points." (15 rep.)

"Meaning grew up after few stimuli had been given." (15 rep.)

"Two perfectly discrete touches; then the first one had a little tail which seemed to move on the skin in direction of second; then arc movement appeared....." (15 rep.)

"At first touches punctiform, then after a while got arc movement between the two points." (10 rep.)

(T) "Movement very definite toward end of experiment." (10 rep.)

"Very slight at first, became stronger toward end." (10 rep.)

The conclusion that haptical movement phenomena are subjectively conditioned is further borne out by the processual results. The dual instruction, to note meaning and process and to give a full report of both, laid a difficult task upon the *Os*, with the consequence that the descriptions of process were frequently sketchy and incomplete. Yet, sketchy and incomplete as the descriptions are, an analysis reveals the following:

(1) The arc, loop, or bow movement, whether unimembral, bimembral or full, is a meaning added to the primary pressure sensations.

(D) "Loop movement, high and rapid. Attending for the movement. When I attend to the sensations the movement disappears and I have only two neutral pressure sensations in consciousness. Movement best obtained by falling into stimulus-error."

(D) "Movement in high arcs. If I attend to the pressure sensations I do not get the arcs, just discrete pressures."

(G) "Movement in high arcs. If I attend to the pressure sensations alone I do not get the arcs, just discrete pressures."

(G) "Got the idea of movement in arcs simply through eye kinaesthesis. When I inhibit this and attend to the pressure sensations, get punctiform pressures, perfectly discrete."

(G) "Two touches and an arc movement between them. Movement is not given in experience, it is inferred. Course of movement can be imaged in either direction."

(G) "Back and forth movement, inferred from visual and thoracic images. As far as experience goes there are simply two pressures."

(2) The processes which carry this meaning differ for the different *Os*, and for the same *O* at different times. At times the accruing processes are visual, at times they are kinaesthetic, and at other times a composite of the two, as the following examples show:

(D) "The pressure sensations are accompanied by visual images of line-like loops which are grey in quality like the grey of a pencil mark on white paper. This grey is weak at first, but it becomes stronger and more distinct as the experiment progresses. The bow was not complete at first, but with every successive stimulation it became more complete, until the arc was united in imagery. Visual images became clearer and more complete. Coordinate with this, vague and indefinite kinaesthetic sensations or images of head and eyes moving back and forth in rhythm with the stimulation."

(D) "Complete loop, very high and rapid. The cue for movement came from the changing pressure gradient and the meaning given by the visual and kinaesthetic imagery which supplemented it. The visual imagery was of a loop greyish in quality, much like the grey mark of a pencil on white paper. Kinaesthesis of eye movements along arc of this loop; think there were also kinaesthetic movements of nodding of head in rhythm of movement."

(G) "Discrete at first, then grouped in pairs, then peripheral arc movement. Positive of movement, it is there, but there is no sensory basis; given entirely by visual image of an arc with its terminals on the skin."

(G) "Backward movement, 2-1, meaning carried by eye-movement and perhaps thoracic pressure."

(G) "After series had gone on for a little while I got notion of movement in arc, large loops between terminals, carried by thoracic pressure and eye movement."

(G) "At first two perfectly discrete touches, did not think of object making them. After series ran awhile got movement between the two points. Principal thing was eye-movement, tracing arc in imagery."

(3) When the movement is in or on the skin the perceptions appear to be of a different kind; the meaning is inferred from the pressure sensations themselves, and the existential correlate seems to be a spatial and temporal integration of pressure.

(D) "Seemed to slide along skin..... a rapid peripheral extension of the pressure quality."

(G) "Slid on skin, change in extent of experience in time."

Summary

(1) We discovered in the Preliminary Experiments, which were undertaken to identify the 'bow' movement and to determine the optimal conditions under which it appeared, (*a*) that our objective conditions were alone not adequate to the perception of movement; (*b*) that we were

dealing with a perception which is in part dependent upon subjective conditions; and (*c*) that we should have to establish the 'meaning' attitude if we wished to continue the investigation.

(2) We found in the Main Experiments, in which we endeavored to establish the necessary attitude indirectly by rapidly repeating the bimembral stimulus, and by asking in the instructions for reports of process as well as for reports of meaning, (*a*) that various kinds and types of movement, unimembral, bimembral and full, were reported by all the *O*s; (*b*) that the optimal condition for the arousal of the perception varied greatly from *O* to *O*; and (*c*) that the diversities as well as the uniformities in the results of the different *O*s indicate that the perception of bow-movement haptically aroused is not primarily dependent upon objective conditions. The essential requirements seem to be that the *O*s shall have the idea of movement and that this idea be given time for realisation.

(3) The analysis of the introspective reports corroborates these conclusions. (*a*) The perception is gradually built up; (*b*) the arc, loop, or bow movement is a meaning added to the primary pressure sensations; and (*c*) the processes which carry this meaning are associated visual and kinaesthetic images, or incipient sensations.

(4) When the movement is in or on the skin the perceptions appear to be of a different kind. The meaning is inferred from the pressure sensations themselves, and the existential correlate seems to be a spatial and temporal integration of pressure.

SOME RECENT PSYCHOANALYTIC LITERATURE

By G. Stanley Hall

A Young Girl's Diary. Anon. Pref. by Sigmund Freud. N. Y., Seltzer, 1921. Pp. 284.

This diary was begun by a Viennese girl of eleven, and continued until she was fifteen and a half. She belonged to the well-to-do and intelligent bourgeoisie, and was evidently a girl of a somewhat precocious type, not unusual in our day. The most remarkable thing about these very secret and confessional records of incidents and spontaneous feelings is that so large a part of all the child's interests, gossip, and activity was to find out more and more about the sources of human life. Every item of this knowledge which filtered in was pooled with that of her most intimate chums, reasoned about, and made the center of all kinds of emotional activity, so that the volume might almost have been entitled A Girl's Four-Year-Old Struggle to Understand Sex and Reproduction. It all illustrates in a rather too exquisite way Freudian theories of the immense travail of soul involved in the *Aufklärung* and we have hints in it of about all the aberrations of the *libido* which are described in his writings. Freud says that nothing has ever been written "enabling us to see so clearly into the soul of a young girl during the years of pubertal development." "The little author is a literary artist." In the diary, he adds, we see how interest in sex-life first arises vaguely and "then takes entire possession of the growing intelligence, so that the child suffers under the load of secret knowledge but gradually becomes enabled to shoulder the burden."

Scores of entirely innocent and neutral words (secret, figure, understanding, illness, disease, relation, period, part, and many more) suddenly become centers of intense self-consciousness and curiosity, common knowledge of these being the basis of friendship and its impartation being friendship's chief function. From a sense at first of being shut out from all that made life really interesting, this child, by incessant prying and searching, slowly came to feel that she had a very superior knowledge and even had a mission to initiate others. An adult will be amazed to find how many partial faults and misleading concepts are possible in this field. If this is the most important of all kinds of knowledge, this girl was a genius and a superb psychologist without knowing it. She penetrated to, and gave away more completely than anyone else, the secret of her sex during the period of its most rapid development.

The questions that will inevitably arise in every reader's mind are: first, whether the child was normal; and secondly, whether her surroundings in Vienna did not bring her to very unusual envisagement of all sorts of improper things. In any case, the book is a remarkable contribution to "flapper" psychology, of which we know so very little, and takes its place beside the revelations of Marie Bashkirtseff, Mary MacLane, and Una Mary. It should be added that, if the book were radically expurgated, there would be enough left to make very interesting and stimulating reading for girls of like age and their parents, although it would be greatly reduced in size. But as it stands, it would be a grave mistake to allow

it to fall into their hands. While cultivated parents of neurotic girls of this age might be helped by it, its circulation should be chiefly among the medical profession. The expurgation of a dozen or a score of pages would greatly help.

The Psychology of Daydreams. By J. VARENDONCK. Introd. by SIGMUND FREUD. Lond., Allen and Unwin, 1921. Pp. 367.

Dr. Varendonck, a brilliant young Belgian student best known by his study of children's societies, was for three years during the war an interpreter for the Allies and writes his book in English. He had read little but Freud's "Dream Analysis" before he entered the war, but after some years succeeded in getting hold of the mode of thought-activity which has been called autistic or fore-conscious or, by Jung, undirected thinking, which is best studied just before going to sleep. The moment he becomes aware of these dreamy states he concentrates upon the last link of the chain, and by dint of long practice has been able to drag up previous links, so that he gives us, based very largely upon his own personal experience and self-study, by far the best picture of revery, which shows inner mental life in its estrangement from the outer world. Hysteria is the invasion of the system of motility by unconscious reveries. The paranoid gives these reveries reality. The productions of revery are much more accessible because the inner self does not drift so far from its outer conditions. In the early part of the book the author has, to his mortification, to give us many personal details that we may understand his reveries, but later these are supplemented by the reveries of others and the personal element fades. These studies convince him that voluntary thinking is a recent adult acquisition, and that in revery we are thinking as the child or as primitive man thought. In conscious thought we are able to eliminate affective processes, but these dominate in the fore-conscious. The latter, again, has a very highly developed symbolic character and can never be abstract. It is also egocentric and is rarely entirely impersonal. Like dreams, reveries often center about unsolved problems and cares, and their end-exteriorization is of a more immediate and topical character. They are almost always adaptive and in a sense teleological. And yet revery and play, as interpreted by Groos, have much in common. The censor is less active than in directed thinking, but more so than in dreams. Conscious activities are all assisted, or should be, by affective thinking. Conscious thought, however, is characterized by far greater freedom from the defects engendered by emotions, and should perhaps be characterized as the elimination of all affectivity. It is under the dominion of volition only. It alone can be truly speculative. The author's conclusion is that unconscious, fore-conscious, and conscious thinking are three manifestations of the same process varying only in degree of function which, originally regulating the relations of the individual with the outer world, constitutes a manifestation of universal energy and is as eternal and unceasing as the other organic activities in the service of adaptation.

The New Psychology and Its Relations to Life. By A. G. TANSLEY. Lond., Allen and Unwin, 1920. Pp. 283.

This book claims to be the only one in English which has attempted to gather all the light shed by psychoanalysis upon the behavior and treatment of normal individuals. All the factors characteristic of the mentation and behavior of the neurotic are at work in the normal individual, whom the analyzer does not see and whom he too rarely considers. This gap the author seeks to fill by giving what he calls a biological view of the

mind, with selections from the literature that was most helpful, but with no attempt to deal with the psychopathology proper. Accordingly, after an introduction briefly characterizing "the new psychology," the physical and psychical worlds, he passes in Part II to the structure of the mind, specific responses, typical mental processes, the unconscious, and complexes. Under "mental energies" he discusses *libido*, which he identifies with interest, equilibrium, and sublimation. The byways of the *libido* are suggestibility, failure and regression, conflict, forgetfulness and repression, dreams, projection and idealism, psychical segregation and displacement. Part V treats of reason and rationalization and its relations to conduct; while the last Part, dealing with the contents of the mind, characterizes the primitive instincts, the great complexes, especially that of the ego, the partial and universal herd, the sex instinct and the primary sex complex, byways and combinations of the sex instinct, and the interpretation of the universal complexes. As a whole, the work is undoubtedly the best introduction to the subject for the general reader whose interest is not primarily in abnormalities.

Psychoanalysis and the War Neuroses. By S. FERENCZI, K. ABRAHAM, E SIMMEL, and E. JONES. London, 1921. Pp. 59.

There has been a very strong and growing conviction among the great majority of physicians who dealt with the psychic traumata of the great war that the sex factors on which the Freudians laid so much stress had little or nothing to do with the causation or the cure of these cases, but that they were purely of ontogenetic origin and due to fear, conscious or unconscious. This little volume, with an introduction by Freud, attempts to convince us that this view is erroneous, that the neuroses of war and peace are not fundamentally different, and that obscure sex factors enter even into shell-shock, in various cases of which we have "genitality" shown in various symptoms. War-neurosis is simply a traumatic neurosis such as was well known to occur after fright or severe accidents without any reference to an ego conflict. The *libido*-theory was put forward by Freud only with reference to the transference of the neuroses from peace conditions.

Ferenczi's article in this symposium is extremely valuable as containing a survey of all the very voluminous German literature bearing upon the subject. Indeed, in no other of this author's writings known to us are we so impressed with his breadth of view and knowledge and his mental activity in coming to terms with so many different shades of conclusion. He shows that many German specialists have accepted very many of Freud's conclusions apparently without knowing it, and that despite the violent opposition of the Oppenheim group. Even the marked regressive character of all war neuroses, shown at the conclusion of peace, had been described by Freud before the war, although he was speaking only of accidents.

Abraham in his contribution thinks that the war traumata act on the sexuality of many people in the sense that they give the impulse to regressive alteration which endeavors to reach Narcissism. The soldier must always be prepared for unconditional self-sacrifice in favor of the mass, and this signifies the renunciation of every vestige of Narcissistic privilege. In the unconscious of many we do not suspect of Narcissism slumbers a belief that they are somehow invulnerable or immortal, and an explosion or wound suddenly destroys this belief, so that the security they felt collapses into a feeling of powerlessness and then the neurosis sets in.

The best of these papers is by Simmel, who has really nothing to say of the *libido*-theory, but describes his own war-experiences when he was in charge of a special hospital for the war neuroses, and finds Freud's

views in regard to the unconscious psychic causation and cure to be abundantly confirmed. The victim takes refuge in his symptoms, not for the purpose of preserving his physical but his psychical existence. There are very few war psychoses but very many neuroses. The conditions of the soldier's life involve the constant narrowing of his ego complex. He is an inconspicuous unit in a vast whole, must have no will of his own, and the narrowing and suppression of his consciousness represent the initial stage of the war neurosis and consciousness may be lost suddenly. Then the unconscious has its innings. By hypnosis he can be made to live through his experiences, and this was found to be very effective. Doctors who devise systems of torture, hunger cures, dark rooms, prohibition of letters, painful electric currents, etc., to compel patients to abandon their neurotic symptoms, really recognize Freud by inversion of his fundamental principles, *i.e.*, they make the patient wretched to force him to flee into health.

Jones' articles is more controversial, and is a defense of Freud's theory of the neuroses. War itself is an explosion of forces that are in conflict with the standards of civilization. "It is an official abrogation of civilized standards" sanctioning barbaric activities. He tells us that the readjustments necessary in war are "by no means so difficult as can arise in various situations appertaining to the field of sex." He takes his departure from Narcissism, and suggests that not only sex suppressions involved in war but wounded self-love, the severity of discipline, the imminence of danger and even death, and fear, which is the thing centrally to be considered (because, as Freud teaches, all psychoneurotic symptoms are constructed to prevent the development of fear and anxiety), will be found, when we have fully understood the war neuroses, to be the key to the explanation of all symptoms.

A Psychoanalytic Study of Manic-Depressive States. By LUCILE DOOLEY. Psychoanalytic Rev., 8, no. 2, April, 1921.

This is a very interesting and critical study of five cases of a mental symptom-group which it has generally been supposed psychoanalysis cannot help; but the author concludes that, while in general the psychoanalytic results have been meager and doubtful, there was material assistance, especially in three cases, which had not however become chronic. In one there were real alternations in cycle although there was little likelihood of permanent recovery. In another, a bigoted, self-willed character, there was little help because the patient did not cooperate. None of the cases had much intellectual training, and this is very important, as has often been pointed out, especially for securing the needed attention and interest. Four out of the five cases had reached puberty at an unusually early age, and all had developed sex repressions as a result of the mother's failure to meet their needs at the critical time. There was unsatisfied curiosity, doubt, and fear before twelve years, when the patients were unable to meet their problems without help. Thus all four patients who married did so with lack of self-control, excessive bashfulness, modesty, prudery, incipient homosexuality, so that their marital relations were unhappy. Their delusions were usually attempts to fulfill regressive wishes, and it was possible to trace the stages of regression step by step to deeper and deeper layers of the unconscious. Thus the manic-depressive type does seem, contrary to the usual conceptions, sometimes to descend to levels as low as those reached by dementia praecox. The manic-depressive character is extroverted, always trying to relate itself to the environment, but minimizing the subjective element. The behavior of manic attacks is evidently a defence reaction, and in a depressed phase offence is no longer possible under profound consciousness of defect. Hence the difficulty of psychoanalytic treatment. The patient cannot bear to hear the truth.

Dangerous Ages. By ROSE MACAULAY. N. Y., 1921. Pp. 242.

This novel has a unique psychological scope and importance, describing typical women of four generations living together at the ages respectively of just over 20, 43, 63, and 84. The great-grandmother, of the latter age, is made an oracle of sense and discretion in emergencies, but her advice is always resisted. The story opens on the mother, aged 43, who has brought up a son and daughter to maturity and now wants to revert to her medical studies, being discontented with the social and domestic duties she has done so well, and not wishing to be merely a helper to her husband but to have a long-postponed career. She takes up her studies, but finds her mind too stiff as compared with that of her son, and in the sequel, after long delay and a good many episodes, reverts to being a wife and mother. The grandmother, 63, is a no less interesting study. She never had culture aspirations but was a good although bigoted family mother till her husband died and her children married, when she felt aimless, set aside, and finally drifted to psychoanalysis, which has been a fad of both her children and grandchildren, and despite many shocks became devoted to it and was given a new life by it. She collapsed, however, when she could no longer afford her bi-weekly seances with the specialist to whom it was a great satisfaction to her to pour out her soul. The granddaughter painted, wrote poetry, abhorred everything Victorian, faced even the most delicate questions openly and with an almost stunning frankness, and fell in love with a fine young man whom she had won away from her older aunt. She had free-love ideas and waged a very long and persistent argument with the lover against being tied by the marriage bond, yielding to his old-fashioned scruples only when she found she was otherwise certain to lose him. Perhaps most interest centers in the aunt, Nan, a successful novel writer, wonderful swimmer, bicycler, ultra-emancipated hyper-Freudian.

The moral of the book is that the four principal characters illustrate four very critical stages from which all, after a more or less prolonged period of circummutation, emerge into sanity. The style of the book and the author's interpretation of psychoanalysis are remarkable.

Dream Psychology. By SIGMUND FREUD. N. Y., McCann, 1921. Pp. 237.

The present reviewer of this work finds himself baffled to know who made the book. Freud has so lately published his "General Introduction to Psychoanalysis" that it seems hardly likely he would so soon write another work like this. Morever, the announcement of the publisher, J. A. McCann, says, "Here is presented to the reading public the gist of Freud's psychology in the master's own words and in a form which shall neither discourage beginners nor appear too elementary to those who are somewhat advanced in psychoanalytic study." In Tridon's colorless and ineffective introduction one finds no light shed upon the subject. The book is certainly not made up warp and woof of quotations, and what the reader will want the publisher to explain is what is meant by the phrase "in Freud's own words." How much, if anything, did Freud have to do with this, and who is responsible for its publication? It is by no means without its use and merits, but it does not strike the present reviewer as being the kind of digest Freud would have made himself, and he thinks that the reader should know Freud's real relation to the work.

The chapter headings are: Dreams have a meaning, The dream mechanism, The dream disguises, Desires, Dream analysis, Sex in dreams, The wish in dreams, The function of the dream, The primary and secondary process—regression, The unconscious and conscious—reality.

Man's Unconscious Spirit. The Psychoanalysis of Spiritism. By WILFRID LAY. N. Y., Dodd, Mead, 1921. Pp. 337.

This is the fourth book of the author on psychoanalysis, and here his thesis is that psychical research is on the wrong track. Mediums refuse to be analyzed "because unconsciously aware of the unconscious deception that they innocently practise." All so-called messages are really from the medium's own unconscious storehouse of memory images. So far as science knows, spirit is nothing. "There is no such thing to be revealed as a force operating from without upon real things with anything more like human intelligence than the swelling of water before it becomes ice." There are no breaks in the universe. "There is a wild attempt to guess out what will please the hearer without any attempt whatever to gain true breadth of wisdom and reality of thought." "Attempts to gain the *imprimatur* of science for the unconscious utterings of second rate minds have resulted only in the impartial and broadminded observer being repelled," etc. Evil messages are the pitiful dejecta of the unconscious of certain individuals. Instead of trying to prove spirits, men should recognize that this effort is only infantile. "The wish for proof is the direct result of the fear of death."

From this point of view all the eleven chapters are written. They are on the stream of consciousness, emotions, psychoanalysis, the unconscious as an urge, mechanism, unconscious emotions and the will, belief before knowledge, knowledge above belief, man's unconscious spirit, scientific investigation, the present status. The author's main thesis is only what almost every really scientific psychologist has long held, and is substantially that set forth by the writer of this note thirty years ago in the early volumes of this *Journal*. There are a number of striking new illustrations in the book, and it is easy and attractive reading, but it contributes little that is new to those familiar with psychoanalysis.

Getting What We Want. By DAVID O. EDSON. N. Y., Harper, 1921. Pp. 287.

The twenty-one chapters of this book might have been written as syndicate press-articles, for the author's sprightly style suggests Frank Crane. The sub-title of the book is: How to apply psychoanalysis to your own problems; but there is no wearisome reiteration of familiar Freudian nomenclature, and no effort to bring the interesting cases outlined under the classical rubrics of the analysts. The author's theories are strongly tinctured with anthropology, and the contrast between the dark and perhaps hook-nosed thinker and the blond blue-eyed doer is constantly in evidence. Indeed, these differences seem fundamental in the writer's diagnosis and therapy. There are four stages of human development; the archaic, dominated by hunger and love; the auto-erotic, which began with higher apes; the Narcissistic; and finally the social. Everybody wants to be great, but success in life largely depends upon determining the proportion of blond and brunet components in our make-up and regulating life accordingly. It is impossible to epitomize such a book. The analyst's work largely consists in seeing to it that men do not try to plow with a limousine or go on a picnic with a high-powered tractor. The man who inherited great wealth and came to the doctor to be cured of drowsiness is a typical case of the misfit in life of a psychic mechanism. In him every wish had been gratified except the wish to be great and to do something himself, and to this the routine of office life which he had to keep up gave no vent. He therefore took refuge in daydreams full of achievement, and in the summer vacation, when the archaic instinct of the old Vikings in him found expression in his yacht, he never complained of the drowsy feeling.

The *pièce de resistance* in the belated July number of *The International Journal of Psychoanalysis* is an autobiography, with comments by Dr. C. M. Haviland, written by a young man of twenty-three in one of the United States training camps on the eve of his departure for the battle front of France, compiled as a gift to a friend in case the writer failed to return. It breaks off abruptly, apparently in his eighth year, and his intention to bring it up to date in France was always frustrated. He wrote it, or rather he says it wrote itself, under the greatest emotional stress.

One of his earliest and most persistent concepts, when he was about three, was that he was of immense size and lived and moved inside a correspondingly large crystal sphere. He himself was an occultist, and believed this to be a reminiscence of a previous state of existence. Haviland, however, interprets it as a memory of prenatal life in the sphere of the uterus. It is an image "coming from the unconscious and thrown upon the screen of consciousness." To this sphere he was prone to retreat, for here he was absolutely monarch, and apparently he had not outgrown, even at the time of writing, a very vivid memory of it. At the age of four he had a distinct sense of the presence of another child, whom he often imaged as a white-robed guide, who at first directed him entirely in one of his most elaborate structures, *viz.*, the building of a sewer in his imaginary kingdom, later was consulted about everything, and finally slowly faded with years. This presence Haviland interprets as at first a mother-image, which had to be made masculine, and finally became identified with self. The child also developed an apparently purely original language in which he communed with the mysterious presence and also with the many two-inch people with whom he populated a wonderful fairy kingdom. His addiction to this language caused, for a time, the suspicion on the part of his parents that he was abnormal. The fairy kingdom was developed in great detail and had its own king, who was killed by the Crown Prince, himself later conquered and imprisoned in a high tower by an army led by an obscure boy within the province. The queen, whose features and traits were very vividly imaged, enlisted his deep sympathy. The countess, who was represented by a repulsive toad and was the chief mourner, dived from the royal barge and escaped death by swimming under water. This Haviland, we think rather lamely, interprets as a manifestation of the Oedipus complex. He makes no attempt to explain the very elaborate grave-yard in which all dead or dying animals were interred and where even insects were buried *en masse*; nor the persistent propensity to model catlike forms, a girl's pet and expressing his feministic tendencies; nor does he explain the persistent horror of all large animals and even of the barn in which they were kept. We should have welcomed some further statement as to the present state of this most interesting patient, whose childish imagination was so extraordinarily creative, illustrating spontaneous autistic powers quite as remarkable as those of Una Mary, Bashkirtseff, Mary MacLane, George Sand, etc. Analysts tend to explain such phenomena as due to an impulse of retreat toward infantile and even prenatal conditions.

Outwitting Our Nerves: A Primer of Psychotherapy. By JOSEPHINE A. JACKSON and HELEN M. SALISBURY. N. Y., The Century Co., 1921. Pp. 403.

To one who is often asked to suggest a lucid and sprightly introduction to psychoanalysis this book is a godsend, for it best serves its purpose, as indeed, since it is the last of a long series, it ought to. The authors have had a long experience in dealing with diseases where there is "nothing to be cut out and nothing to give medicine for." And yet these troubles seem to be more common in the world today than those which are helped by surgery or drugs. In the sixteen chapters here we have not only a clear statement of principles but also many well-chosen illustrative cases and a brief epitome of the theories, methods, and results of psychoanalysis, with hints at its larger culture-significance and the latest contributions to the subject. The literary quality of this book should make it very popular.

REVIEWS OF BOOKS

Grundzüge der Reproduktions-Psychologie. By BENNO ERDMANN. Berlin und Leipzig, Vereinigung Wissenschaftlicher Verleger, Walter De Gruyter & Co. 1920. Pp. viii, 186.

To those who are acquainted with Erdmann's contributions to psychological literature, as well as to those who have attended his lectures on psychology, the main argument of the *Grundzüge* will not be new. The book presents a careful restatement and logical completion of his scattered discussions of a fundamental psychological process. The original discussions appeared in his numerous papers on thought and language, in his logical studies, and in his *Theorie der Apperzeption.*

To those who believe in the essential soundness of his account of the reproductive processes in perception and in thought, this restatement will be a welcome and convenient document. But the book was written for those who are not familiar with his doctrine. It was obviously motived by the desire to present a fundamental portion of his logical and psychological contributions in a form that would insure it permanence and a fair hearing. Erdmann makes very clear how much importance he attaches to it.

The book was obviously produced under difficulties. Controversial matter, originally planned for, was cut out bodily. The rest bears marks of condensation. The publisher is formally thanked for producing the book in spite of the "unfavorable times."

Notwithstanding its comparative brevity, the *Grundzüge* contains a careful presentation of the personal and historical conditions of its argument. A frank statement of epistemological and psychological postulates, defence of "arm-chair introspection," and a remarkable attempt to represent his underlying personal equation, are added to the more commonly expected acknowledgments to his teachers and associates, to his historical antecedents and to experimental data. The effort to give the reader all the information necessary for discounting the bias of the author, as well as for a critical estimate of the mental antecedents and personality behind the doctrine, is unparalleled in psychological literature.

If one sought the most fundamental principle of the *Grundzüge* it would probably be found in the doctrine of non-independent reproduction by apperceptive fusion. This is an immediate, necessary, and universal process in every perception, and consists of the arousal of certain representables and their fusion with the direct effects of stimulation. It should not be confounded with the associative fusion of sensory data into wholes possessing qualities, or with the associative interweaving of percepts by preestablished neural paths, by personal experience, or by similarity. It is the precondition of them all.

Any *Reproduktions-Psychologie* must start with the postulate of psychophysical residua. Their nature still remains a matter of debate. That they exist as unconscious dispositions to new moments of consciouaness, that they retain as unconscious dispositions the associative interweaving of the original experience, there is abundant evidence in habit, both motor and perceptual, intellectual and emotional. The possibility of reproduction which is not conditioned by preformed paths or by experience; whether actual contents of consciousness are the exclusive conditions of reproduc-

tion; the possibility of reproduction which does not appear as conscious content; these, together with the function of attention in reproduction, are the main problems of the *Grundzüge*.

Sense-perception regularly involves memory-factors for which there is no direct sensory stimulus. These are commonly associated supplements. The consequent perception may be called associatively supplemented (*ergänzt*) perception. A still simpler and more fundamental supplemented perception is found in tachistoscopic experiment and occasionally in daily life. It occurs when attention is concentrated on the perceptual content, and also when objects are casually noticed in cases of diffused attention. In spite of the narrowness of this kind of perception, it is often clearer than the present stimuli can account for, and it commonly involves an identifying cognition. Either fact would imply the interaction of reproduced sensory experience. The notable peculiarity of such reproductions is that they never appear in consciousness independently, but always fused with the immediate results of stimulation. The term apperceptive fusion, which may be applied to such reproduction, must not be confounded with associative fusions of conscious factors. It refers not to conscious contents but to the conditions of consciousness. Apperceptive fusion involves two moments which may be called respectively the stimulus-component and the residual component. In any given fusion the two components are simultaneous. Dynamically, the stimulus-component is primary. The residual component, however, is responsible for the fusion.

Apperceptive fusion is the condition of all cognition. It determines the course of attention and is the cause of the illusions of normal and abnormal life. All cognition is recognition. No perception (even the most undeveloped) is entirely free from apperceptive fusion. In adult consciousness it underlies the serial development of observation, introspective as well as sensory.

The reproductive processes that begin in apperceptive fusion commonly lead to mediate supplemental interwoven reproductions. Of these, remembered, abstract, and imagined presentations are the simplest forms. Such reproduction is associative, but not in the sense of Hume's association of ideas. Only residua are associated with the immediately aroused component of apperceptive fusion through which they are reproduced. In mediate supplemental interwoven reproductions neither the associated residua nor the condition of their reproduction is a conscious content.

Of the various forms of supplemental interwoven reproduction the most momentous is the perception of symbols. The cognition of symbols presents every form of sensory cognition from apperceptive fusion to the more complicated thought-processes. In the discussion of these supplemental reproductive interweavings, Erdmann restates, partly in the form of equations, his contributions to the interrelation of thought and speech, as well as to the psychological organization that underlies formulated thought. The argument is too condensed for recapitulation.

Not only may reproducing moments be non-independently aroused residua, now fused with sensory moments, and again interwoven as associated supplemental moments, but also the products of reproduction may remain unconscious though stimulated. This occurs in apperceptive preparation for (or in attention to) a coming unknown stimulus, in the silent elaboration of speech, and, as Erdmann's self-observation indicates, in the lack of meaning-consciousness antecedent to familiar utterance. The understanding of sense impressions, of speech, and of reading matter may on occasion involve wide-spread conscious reproduction of agglutinated residua. When the material is sufficiently familiar, the stimulated agglutinated residua may remain unconscious. These unconsciously stimulated residua may be represented in consciousness by emotional states, of which the feeling of familiarity is an example.

The climax of apperceptive completion appears in the sublogical processes such as abstraction, comparison, expectation, and combination, which are the psychological foundation of formulated thinking both inductive and deductive.

Erdmann regards attention as a ground-function of the mental life. Whether it takes the form of clearness of the changing content of consciousness or the form of expectation, it involves a reproductive process which is determined by the connection of residua. In expectation that which is expected is commonly not given as an object of consciousness, though under favorable circumstances it may be. In the former case what is expected must be regarded as an excited representable, a kind of preconsciousness.

It is always a dubious process to present discontinuous bits of a careful discussion for approval or disapproval. In the present instance it were worse than useless unless it induces the reader to explore for himself the original mine of fine observation and far-reaching analysis.

RAYMOND DODGE

Wesleyan University

The Origin and Development of the Nervous System from a Physiological Viewpoint. By C. M. CHILD. University of Chicago Press, Chicago, Illinois. 1921. Pp. xvii., 296.

In this book the origin and development of the nervous system are considered from a physiological point of view.

The first five chapters form an introduction to the primary thesis, and are devoted to a discussion of pattern in the organism and the possible relation of pattern to the physiological gradients in general.

Protoplasmic pattern and organismic pattern, in the author's opinion, differ probably only in the order of magnitude; and the question immediately arises whether organismic pattern (the relation of parts in the organism) is inherent in protoplasm, and develops spontaneously, or is, in some sense, a response to environment. The subject-matter of the book is concerned with an attempt to answer this question. Excluding the purely contactual or mechanical factor, there are two chief categories of relation between protoplasm and its environment, the material or chemical and the dynamic or excitatory; and the latter—the excitation-transmission relation—the author believes to be the important factor in impressing the organismic pattern on the protoplasmic substratum.

He discusses the physiological basis of various pattern-types in plants and animals, attaching great importance to the physiological gradients (quantitative gradations in functional activity) in pattern-production.

The nervous system, *the* organ of integration, in its origin and development, does not involve the appearance of a new functional activity different from the fundamental activities of protoplasm in general; some kind of physiological continuity exists between excitation and conduction in protoplasm generally, and the development of the nervous system. "Living protoplasm is functioning at all times and development is a process of functional construction, that is, beginning with a given structure and function, the continuance of function modifies the structural substratum, and this in turn modifies further function, and so on." According to this conception, the author believes that the nervous system is the physiological and morphological expression of the excitation-transmission relations, first with respect to the primary or chief physiological gradients, and later with respect to the progressive developmental complications as they arise.

The localization of the nervous system is an example of surface-interior pattern, and the general direction of growth and differentiation is down the physiological gradients, beginning at the anterior end and extending posteriorly.

To account for the origin and development of the neuron-pattern the author suggests that this may be determined by the electrical polarization of the cell (neuroblast), which must lead to changes in its physiological condition and activity. In the stimulation of living protoplasm generally the primary change is probably electrical and this electrical polarization of a neuroblast may determine changes in its rate of metabolism and consequently in its physiological activity.

In a discussion of this question at the present stage of its development we are hampered by a lack of knowledge of the fundamental nature of physiological processes and of the ultimate structure of biological organisms. Although the mechanistic conception of life may not appeal to all, mainly on account of the paucity of facts which can be brought forward to support it, yet the conclusion of the neovitalist that the relation between pattern and process will never be understood is unjustifiable. There is little doubt that the two have a constant and necessary relationship, and for his attempt to elucidate the possible character of this relationship in the origin and development of the nervous system the author is to be commended.

SUTHERLAND SIMPSON

Cornell University

Fünf Reden von Ewald Hering. Herausgegeben von H. E. HERING. Mit einem Bildnis von EWALD HERING. Leipzig, W. Engelmann. 1921. Pp. 140. Price about 75 cents.

Dr. Hering has earned the grateful thanks of all experimental psychologists by this reprint of certain classical addresses delivered by his distinguished father. The addresses are: Ueber das Gedächtnis als eine allgemeine Funktion der organisierten Materie (Vienna, May 30, 1870); Ueber die spezifischen Energieen des Nervensystems (Prague, [1882] 1884); Zur Theorie der Vorgänge in der lebendigen Substanz (Prague, [Feb. 18,] 1888); Zur Theorie der Nerventätigkeit (Leipzig, [May 21, 1898] 1899); and the Antwortrede to the award of the Graefe-medal made at the 33d meeting of the Ophthalmological Society (Heidelberg, [Aug. 6,] 1906). The text has apparently been edited, so far as editing was needed; I notice the removal of a troublesome clause from the third address (p. 64). The book is printed in large and clear type on paper of a rather poor quality. The frontispiece portrait is excellent.

There are some curious omissions. There is no paged table of contents; the fifth address does not figure in the list on cover or title; and we are not told where the addresses were originally printed. The Open Court translation (1895) of the first two—those on Memory and on Specific Energies of the Nervous System—is similarly silent. The address on Memory appeared in the *Almanach der Wiener Akad. der Wissensch., Jahrg.* 20; a second edition of the offprint was published in Vienna in 1876; and the address was included in W. Ostwald's *Klassiker der exakten Wissensch.* (no. 148) in 1905. The address on Specific Energies was given at Prague in 1882 (see p. 79 of the present reprint) when Hering assumed the rectorate of the university; it seems to have remained unpublished until printed (1884) in *Lotos, naturwiss. Zeitschrift hrsg. v. d. deutsch. naturwiss.-med. Verein f. Böhmen*, vol. v. The third address was also published in *Lotos*, vol. ix. The Leipzig lecture was issued in pamphlet-form by Veit & Comp., Leipzig. I do not know whether the Antwortrede has been published before; it is not contained in the report of the Heidelberg Congress in the *Arch. f. Augenkeilkunde*, 56, 1907, 93 ff.[1] It is, as the editor remarks in his preface, of especial interest, since Hering uses the occasion

[1]The volume is wrongly given as 55 in the *Zeits.* bibliography (xlvii., 1908, 362, no. 654) and in the *Psychol. Index* for 1906 (13, 1907, 41, no. 649).

to review in brief the whole course of his scientific work, and to acknowledge his intellectual debt to Lamarck and Darwin, Schopenhauer and Fechner. We should all have connected him with three of these men, but I doubt if we should readily have thought of the fourth.

E.B.T.

The Physical Growth of Children from Birth to Maturity. By BIRD T. BALDWIN. University of Iowa Studies in Child Welfare, from the Iowa Child Welfare Research Station. Iowa City, University of Iowa, 1921, Vol. I, No. 1. pp. 411.

The author presents a comprehensive survey of studies which treat of physical growth, together with a more intensive study of the growth of (sometimes only a few) individuals taken at intervals over a considerable period of time. It represents a serious, methodical effort to establish norms which shall be based upon the growth of the same persons and not, as is the rule, upon different persons at different ages. Not the least important part of the *Study* is the plea for standard apparatus and for uniform procedure in making measurements. In the case of chart LIII, p. 150, where he gives the weight in pounds, the author departs from his demand for the universal use of the metric system for scientific work. One may or may not agree in the matter of choice of measurements to be taken; but one must admit that the 23 measurements chosen are, perhaps, as important as any others. We especially welcome the inclusion of psychophysical measures. Some might wish to take fewer measurements, and to work intensively till reliable norms are to be established,—thereby denying or ignoring the doctrine set out in the *Study*, which assumes that normal growth is not only a matter of amount but also of relation; that the relation of weight to height is to be considered just as well as weight itself. In any event, the author does well to emphasize the need of case-histories in an endeavor to determine the relation between growth and nutrition, disease, sex, race, geography and environment.

The first section of the *Study*, which deals with instruments and methods of measurement, is well illustrated by photographic reproductions, so that the form of the apparatus and the manner of their use are perfectly clear. Even more photographs would serve the purpose of standardization, although the description may be taken as ample in most cases. Yet on p. 21 we find that "the child's *left* middle finger touches a vertical wall or moulding" and "that the observer applies the square lightly against the free end of the middle finger of the *left* hand:" a statement which is obviously at fault, although one which anybody with sense can correctly interpret; it only emphasizes the importance of illustrations, since standardized procedure is, of all things, the prime requisite. Only a portion of the 23 measurements outlined find a place in most of the tables and charts; only 2 in the charts and tables for babies; 15 in the tables and 3 in the charts for pre-school children; 8 in the charts and 15 in the tables and profiles for school children, while an additional measurement comes into the chapter on anatomical age.

Chapters III, IV and V are replete with tables of averages for groups and of individual measurements, which show the absolute and relative gain in the various physical dimensions for various ages; with charts which give the growth-curves of individual boys or girls in a single dimension; and, of especial note, with the synoptic profiles of growth, which sum up in concise form the growth-history of the individual in 15 dimensions,—although the numerous inversions are inexplicable in terms of any tables given and the author says nothing about them. In the summary at the end of the chapter on infants there are two conclusions which seem to oppose each other. We are told that there is no positive correlation between weight at birth and weight at the end of the first year; in the following paragraph we read that from 60 to 70 percent of the babies which are above average

weight at the beginning of the year are above average at the end of the year and conversely. No figures are given; but it seems that, if 65 percent of the heavy remain heavy and 65 percent of the light remain light, a positive correlation of some degree would surely follow.

All this tends to emphasize the basic notion of the work, namely, the idea that study should be made of the same individual throughout the period of his or her physical development; that we should study the growth of individuals and not of masses. It results that there is no true single norm to which all should conform, but rather that there are general types or forms of curves of growth, and that the individual should approach to type although his curve of growth may rise above or fall below the average. By the time a child has reached school age, if there is a record of his growth taken at semi-annual or even at annual intervals, the course of the curve of growth has gone far enough for a forecast of future normal development.

Although growth should and does approach a norm, for after all children are more alike than different in their development, the notion that growth is an individual matter and a matter of interrelated parts with emphasis on the interrelations finds further amplification in Chapter VI, where we have the inter- and partial coefficients of correlation between 8 traits, 3 of which are psychophysical in nature; the coefficient of variation; the indices of growth, and the percent of increase from year to year. These, especially the indices of growth to which attaches a good deal of importance, help to give a more adequate picture of the total aspect of growth in all its parts and in all its relations. The figures of Table XXVII, p. 146, do not bear out the second conclusion on p. 147, "that for weight girls are more variable than boys at six years of age and six years later."

In Part III we find an unusual distinction made between anatomical and physiological age. The former finds its principal measure in the surface areas of the carpal bones as shown in roentgenograms and as measured by the planimeter. The reader need scarcely be reminded of the author's long and persistent interest in the subject of physiological age in its relation to mental maturity; yet this topic receives, perhaps, the least adequate treatment of any in the book. The sole criterion offered or discussed of physiological age is the age of pubescence for boys and of physiological maturation for girls, from which the author concludes "that girls who mature early are on the average close to the norm or below it," a fact not confirmed by the figures given on pp. 191-192, where we find that all girls who matured at the early ages were well above the average height of the groups of which they formed a part. It may be that the author means that the girls who mature at 11 and 12 years old are slightly below the normal height at the median age of maturation, which age is about 13 years 8 months.

The classification of practically all extant studies on physical growth under 10 main heads with some 60-odd sub-heads, with short paragraphs mixed in here and there pointing out trends, deficiencies and relationships, will form a logical starting-point, a point of orientation, for one about to start work along this line; it can do no more than orientate, but it can do that very well indeed.

The notes of reference, appended to the several extensive tables of Part V, which indicate the race or class of persons measured, may be a sufficient explanation of the wide variation in height found by workers in the same country (these differences amount in some cases to as much as 10 or 12 cm.); but when the difference in growth is as much as that indicated, it becomes all the more evident that not a single norm but a set of norms or limits of normality must be our guide, and that the ratios or indices of growth may be the most valuable measure.

The last, though not the least valuable part of the *Study*, is the annotated bibliography of 911 titles which makes up the bulk of the concluding section. We may say in summary that the *Study* is important, not for what

it completes (for the norms and conclusions will have to receive much verification or modification, since they are in many cases based on too few cases), but rather for the programme of work laid out. Because it is programmatic, rather than final, it holds all the more interest for any one who would carry forward the task set; the task of completely understanding physical growth in all its aspects, relations and dependences.

L. B. HOISINGTON

Poetic Origins and the Ballad. By LOUISE POUND. New York, The Macmillan Company, 1921. Pp. x, 247.

From a study of early poetry and songs recorded in manuscript or passed on orally from generation to generation, the author brings together evidence to support the several theses which she maintains throughout the book. She insists that "there is no sufficient proof that narrative lyrics were ever, anywhere, at any time, by any people, made and sung at the dance." The dance songs of primitive peoples are not narrative, and the earliest English dance songs are not narrative. When "real ballads" are used as dance songs they tend to decay by repetition, and songs used as dance songs do not develop into ballads, but are simplified into some "striking line or formula." Her second contention is that the authorship of primitive poetry is not "communal," but that "the gift of song seems as instinctive in man as the gift of rhythmic motion, not a development from the latter. Children sing instinctively, and they make their own songs, without waiting for the communal inspiration of group dancing." Hence it is reasonable to assume that primitive verse-makers produced their own poetic constructions. The author believes, thirdly, that the ballad appears rather late in literary history, if by ballad we mean a song-tale. The fourth thesis is that "incremental repetition" is not peculiar to the ballad alone but appears in "all types of popular poetry, from nursery songs to revival hymns," and therefore may neither designate the ballad nor furnish evidence of its origin. It is maintained, fifthly, that the "story song is not a primary but a developed type in the evolution of literature," and emerged from a "higher origin than unlettered folk-improvisation." The type of song-tale created among modern cowboys, soldiers, negroes and other groups is relatively inferior; and yet these groups ought to be no less capable than primitive society of ballad production. Finally, the author urges that it is not true, as some assert, that no more ballads will ever be composed. Perhaps no more ballads of the Child type will be produced, but there will be ballads of new types.

In explanation of the mediaeval ballad literature the hypothesis is offered that it may have "emerged under the influence of the clericals, or in something like it."

The thorough manner in which Professor Pound has collected representative primitive literature and her study of it give weight to her contentions, a support which they especially need since they are in opposition to the prevailing theories of the present time. Only the professional student of literature, however, is competent to render expert criticism of the book. The evolution of poetry no doubt throws light upon certain phases of what may be called mental phylogenesis, but it has been outside the author's province to develop her material from this point of view.

H. G. BISHOP

Grundriss der Psychophysik. By G. F. LIPPS. 3te, neubearbeitete Auflage mit 6 Zeichnungen. Berlin & Leipzig, Vereinigung Wissenschaftlicher Verleger. 1921. Pp. 132. 25c.

This little work appeared first in 1903 (167 pp.) as no. 98 of the Göschen collection. In its original form it gave a straightforward and unprefaced account of psychophysics as the border-discipline between psychology and

physics (in the widest sense), based upon an empirical principle of parallelism. The six chapters were entitled: Problem of Psychophysics, Foundations and Range of Psychophysical Parallelism, Qualitative and Quantitative Modes of Determination of Psychophysical Parallelism; Stimulus and Sensation, Feeling and Expression of Feeling, Subjective Perception and Objective Constitution of Spatial and Temporal Forms. The plan is apparent and very simple.

The new edition shows great changes. Chapter I, on the Foundations of Psychophysics, discusses the relation of special science and psychology to philosophy, mental development individual and social, mind, and the relation of mind to body. "The same things," we are told, "which are accepted by the special sciences as self-subsistent and as grounded in their objective existence, and which are regarded by psychology as manifestations of our own living existence, are considered by philosophy as the revelation of the endless, eternal life which underlies the whole of reality and our own self." A far cry from empirical parallelism—of which, by the way, there seems now to be no single word in the book. Mind is the operation (*Wirken*) that comes to consciousness in the exercise of our vital activities (*Lebensbetätigung*). Psychophysics is psychology, a "psychology directed toward the discussion of the connection with the fundamental (physical) determinations of the objectively subsistent."

This introductory chapter occupies 56 of the 132 pages. It is followed by a transitional chapter entitled Vital Condition and its Expressions (*Der Lebenszustand und seine Aeusserungen*). The simple reaction serves as text for sections on the instability of vital condition and on the adequate (mathematical) representation of its expressions. A third chapter (49 pp.) leads us into psychophysics proper, and covers roughly the same ground as the last 130 pp. of the first edition. The treatment of the senses has been greatly condensed; the methods have suffered less. The most important section, systematically, is §11, on *Ordnen und Messen* and *Die Bedingtheit des Wahrnehmens und Empfindens.*

Whether the new plan and the new matter are judged superior to the old will depend upon the reader's special interest. In one respect the present edition is definitely inferior to its original: the legible Roman type has been replaced by two sizes of Gothic, the smaller of which is trying to the eyes.

A Defence of Philosophic Doubt, Being an Essay on the Foundations of Belief. By A. J. BALFOUR. A New Edition. London, Hodder & Stoughton, Ltd.; New York, G. H. DORAN Co. (1921.) Pp. x., 355. Price $5.00 net.

A reprint, with "some trifling verbal alterations and a few notes," of the essay of 1879, which has long been out of print.

An Outline of Abnormal Psychology. By JAMES WINFRED BRIDGES, Assistant Professor of Psychology, University of Toronto. Second edition revised. Columbus, Ohio, R. G. Adams & Co. 1921. Pp. 226.

This directive and mnemonic Outline appeared in 1919; the call for a second edition proves its usefulness. "Sections have been added on the subconscious, on hunger, on abnormal forms of religion, and on several other minor topics; a blank leaf has been inserted at the end of each chapter for annotations; and a subject index has been appended." The writer, we note, still uses 'hypothecate' in the sense of 'assume.'

Introduction à la psychologie: l'instinct et l'émotion. Par J. LARGUIER DES BANCELS. Paris, Payot et Cie., 1921. Pp. 286.

A series of essays, pleasantly written and full of historical information, leading up to the author's Theory of Emotion, already familiar to readers

of the *Archives de Psychologie*. The chapters are entitled: Aim and Methods of Psychology, Body and Mind, Consciousness and the Nervous System, Spinal Cord and Brain, Reflex and Cerebral Activity, Instinct and Emotion. The book will prove delightfully stimulating to the advanced student, but we can hardly regard it—at least for this country—as a useful introduction to psychology.

Readings in Philosophy. Compiled by A. E. AVEY. Columbus, Ohio, R. G. ADAMS and Co. 1921. Pp. xii., 683.

Quotations and references, intended for class-room use with Leighton's *Field of Philosophy*. The quotations cover a wide range, from the Old Testament and the early Greeks down to Russell and Royce.

PSYCHOLOGICAL PERIODICALS

Arch. f. d. ges. Psychologie, Bd. xli., Heft 1 u. 2. F. HERMANN. 'Der Einfluss des Kontrastes auf den Sukzessivvergleich innerhalb eines festen Reizsystems bei Augenmassversuchen.' [Experiments on the successive comparison of horizontal lines, undertaken to determine the effect of the group, *i. e.*, of an obscure background-idea, upon the course of judgment; such effect should show itself mainly by way of contrast. The experiments fell into three groups, each one of which comprised a number (15, 9, 10) of complete series, with as many N, whose component stimuli were thrown together by chance. (1) Absolute impression, which already involves a consciousness of difference, bears more heavily upon the first than upon the second R shown; (2) along with the main tendency to form a mean-idea of the whole group goes a tendency to form partial means of (three) subgroups; and (3) upper deviations from the mean 'normal' idea produce a greater effect than lower deviations, while this idea itself lies lower than the arithmetical mean of the group.] A. MUELLER. 'Beiträge zum Problem der Referenzflächen des Himmels und der Gestirne.' [Reviews some twenty publications that have appeared since the issue of the author's book *Die Referenzflächen des Himmels und der Gestirne* (1918). Not much has been gained; the Göttingen dissertation (1919) of H. Stücklen, however, contains new and valuable ideas. We need further systematic observations, especially observations made on the ocean, and we need a more exact investigation of visual space: several special problems are outlined.] A. KIRSCHMANN. 'Der Metallglanz und die Farbe der Metalle.' [The author repeats his view that all lustre depends upon parallax: surface lustre upon binocular parallax and the parallax of movement, metallic lustre on the parallax of indirect vision. He seeks on this basis to answer the two questions why the strong body-colors of metals are always red or yellow, and not green, blue or violet, and why the strong red and yellow have but small coloring-power in alloys. Both of these facts are accounted for by the hypothesis that a metal is composed of highly transparent, probably crystalline 'particles,' with high index of refraction. The possibility of constructing pseudo-metals from thin sheets of mica confirms this theory.] R. BECK. 'Beruht die gegenwärtwige Vorstellung des Hochgebirges als schön auf einer Aenderung der menschlichen Ideen von Schönheit?' [High places (Sinai, Olympus, Mount of Olives) were honored in antiquity; but the ancients made roads and journeys for commercial and military reasons, not for pleasure. It is social conditions, not human nature, that has changed.] F. GROSSART. 'Das tachistoskopische Verlesen unter besonderer Berücksichtigung des Einflusses von Gefühlen und der Frage des objektiven und subjektiven Typus.' [Experiments with the Wundt tachistoscope. (1) Factors at work during the original process of apperception are mood, familiarity, ideas in preparation, egocentric ideas, subjective form-quality; factors in subsequent readings are feeling, ideas conditioned

by reflection, auditory image, and wrong associations set up in the visual image by the readings. Feeling is of extreme importance for subjective conviction and for the contents of the *Aussage*. (2) For the objective evaluation of important *Aussagen* account must by all means be taken of type. The objective type is passively receptive; the subjective is actively elaborative. The subjective type has, however, two sub-forms, self-activity with criticism and self-activity without criticism; the former may be even more reliable than the objective type. In general, type is of more importance than practice.] G. E. MUELLER. 'Gesellschaft für experimentelle Psychologie.' [Notice of the 7th Congress, Marburg, April 20-23, 1921.]

Zeits. f. Psychologie. Bd. lxxxvii., Heft 5 u. 6. A. PRANDTL. 'Die psychische Leistungsfähigkeit bei wechselnder Disposition.' [Experiments methodically planned to bring out the difference of mental performance in health and in indisposition (consequent on rotatory vertigo) show that the essential condition is intensity of set (*Einstellung*), *i.e.*, of an organisation of the central processes which is directed on distribution of available energies in the sense of a determinate end (*Ziel*), and elimination of useless and actuation of useful reactions. The result throws critical light on studies of fatigue, and permits the 'feelings' of assurance, uncertainty, fatigue, etc., to come to their rights.—These experiments indicate that reproduction suffers but little, impression considerably, from indisposition. Further experiments prove that retentiveness or capacity of repetition also suffers, and thus raise the question whether retentiveness at large may not be more variable than, *e.g.*, James supposed.] H. H. Keller. 'Experimentelle Beiträge zur Lehre vom Wiedererkennen.' [(1) Experiments with mixed series show that, after a 10 min.-interval, nonsense-syllables are better recognized than monosyllables. After a 24-hr. interval this relation is reversed. (2) The former result indicates that the nonsense-syllables receive a *plus* of attention. Experiments with mixed series of ordinary nonsense-syllables and familiar nonsense-syllables remove this advantage, so that the familiar material now takes the lead in recognition after both intervals. (3) The second result suggests Jost's laws. It is found in fact that in the course of 24 hours a given degree of recognitive familiarity of syllables decreases more slowly if the familiarity is of old standing than if it is more recent (analogue of Jost's second law); and that renewed impression has, as regards later recognition, a greater value for the 'older' than for the 'younger' syllables (analogue of Jost's first law).] I. HERMANN. 'Ueber formale Wahltendenzen.' [Experiments with numerical intervals and spatial series, performed on children and adults, normal and insane subjects, groups and individuals, bring to light two tendencies of choice: the one to choose a limiting, the other to choose the middle member of the series. The former is the more primitive; it implies a set for the stimulus and economy of movement; the latter implies something like form-quality, with realisation of equilibrium, stability of the figure, symmetry.] Literaturbericht. Psychologische Gesellschaft zu Berlin. [A committee, consisting of R. Baerwald, M. Dessoir, A. Moll, has been appointed to investigate clairvoyance, telepathy, telekinesis, materialisation.]

NOTES

SYNAESTHESIA IN A CHILD OF THREE AND A HALF YEARS

Edgar Curtis is the son of Professor and Mrs. O. F. Curtis of Cornell University. At the time of this writing he is three years and seven months old. He has never been particularly interested in colors, and he knows only the names of hues of good chroma. He calls rose, and various tints of pink, red. He uses his own descriptive words, however, and he often calls a color reddish, red and orange, etc.

About two months ago his mother noticed for the first time that apparently he has colored hearing. Their home is not far from a rifle range, and the sound of the guns resounds through the hills with a loud 'boom'. One day Edgar asked: "What is that big, black noise?" A few days later he was being put to bed on the sleeping porch. Two crickets were chirping loudly, one of them having the usual cricket-sound with which he is familiar, the other having a very high, shrill chirp in comparison. He asked: "What is that little white noise?" When his mother told him that it was a cricket he was not satisfied, and he said: "Not the brown one, but the little white noise." Then he imitated both of them, calling the lower brown and the shriller of the two white. At another time, when a cricket-chirp uttered from farther away came with a resonant buzz, he called it red.

He calls the sound of the cicada white. The electric fan is orange, and the electric cleaner which has a deep 'burr' is black. The sound of a frog, neither very high nor very low, is bluish. A little Japanese bell is red when rung loudly, and white when it tinkles faintly. A squeaking door is black and white. One could distinguish in that sound two tones of different volume. Drumming on the back of a guitar, when the opening is held to his ear, is black. An engine makes a black noise, but an electric pump is black and white. The low notes of the chimes are brown and black. The shrill crying of a little child is white. The rhythmic rise and fall of the noise made by a street-car in motion is orange. A can is black when it is pounded upon, and when the sound is dulled by touching it with the finger it is red. Thunder is black. A Scotch woman with a broad burr in her speech read him a story, and later he said to his mother: "Do you know what color it is when she reads? It is black."

All the above information has come from the child's casual conversation. He takes it for granted that everyone has the colors that he has, and will often remark: "That noise is red, isn't it?" His parents have been careful not to suggest colors to him, and they have not either suggested that a sound may be of a different color from the one he has named. During a few little experiments, the experimenter sometimes said, "I think that color is white," when Edgar had said it was something else. Every time he was very positive that he was right, and he was manifestly disgusted that anyone could think the sound was white when he had said it was red. He often goes to the piano when he is alone in the room, and to amuse himself touches the keys and tells the colors of the sounds. Notes have been made on those colors when he was not aware that he was overheard. Middle-C is red, and the tones just below are red or red-purple. The bass is black, and the high tones are white. Between middle-C and the white tones are reddish and bluish tones. Edgar never of his own accord named tones yellow, green or gray; but during some later experiments he found tones for them after seeing the color. One day, upon seeing a rainbow, he

exclaimed, "A song, a song!" We thought that this reaction might be a mere matter of association; and we decided to see whether, if he were shown colors, he would find the corresponding tones on the piano.

Red, orange, yellow, green, blue and purple papers of good chroma were used, with the addition of black, white and middle gray. He played with the colors for a few minutes and he was delighted with the idea of trying to find them on the piano. Following are the tones he selected, every color having the tone named and one or two tones above or below.

a''' and all tones above	White
b''	Yellow
e''	Green
e'	Blue
c' (middle-C)	Red
a	Orange
A	Grey
E and all tones below	Black

He selected the tones by playing about on the keys with one finger, and saying, *e. g.*, "This isn't red! This isn't red!" and then gleefully, when he found a tone that suited him, he exclaimed: "This is red, isn't it?" It was interesting to notice that when he was searching for red he did not explore the white or black region, but when grey was given him he went immediately toward the black, and when yellow was given him he went toward the white tones.

We thought that tones of the same musical pitch might possibly be of the same color to him. We found, however, that on the guitar white was *e'*, which on the piano was blue. On the guitar, *c*-sharp was black, though that region on the piano was red and orange. On the guitar, again, *g*-sharp was red and black, while it was red on the piano. One high tone on the guitar was called "a little baby white one."

From Edgar's own adjectives, and from the distribution of the colors on the keyboard, it seems that noises or tones of low pitch and large volume are black or brown or grey, while shrill, high, piercing, thin tones are white; the other colors range over sounds of intermediate pitch and volume. The normal order appears to be orange or orange-red, red, red-purple, blue; then follow, under the experimental conditions, green and yellow. There is some uncertainty as to the red-purples. Our investigation has, however, been so imperfect that such uncertainties were to be expected; it is only the primary and general outcome that we wish to emphasize. We hope that later studies may be made under stricter experimental safe-guards.

ANNA KELLMAN WHITCHURCH

Cornell University

URBAN'S TABLES AGAIN

Typography of statistical tables is so difficult that complete accuracy is long in being achieved. Urban first published his tables for the method of constant stimuli in 1912 [*Arch. f. d. ges. Psychol.*, 1912, 24, 240 f.]. He reprinted them, making two corrections that he had discovered, in the *Praxis der Konstanzmethode*, 1912, 20 f. Then Rich [*Amer. J. Psychol.*, 1918, 29, 121] discovered a third error, and Godfrey Thomson reprinted the tables with all three errors corrected [W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, 1921, 194f.] Now Mr. Howard H. Long of Paine College discovers that the value of $2\gamma P$ for $p = +.73$ should be 0.7551 (exactly 0.755068), instead of 0.7541 as it occurs in all the printed tables. Apparently a typographical error!

This discovery of the fourth error affects Rich's checking tables [*Amer. J. Psychol.*, 1918, 29, 120 f.; Brown and Thomson, *op. cit.*, 198 ff.], which have taken account of the first three corrections. In Rich's tables the value for $p = .73$ and $x = +2$ should be 7.2317 (instead of 7.2307), and the value for $p = .73$ and $x = -2$, 2.2365 (instead of 2.2373).

It may be well to suggest, in case someone again reprints the Urban tables as now corrected by Mr. Long, that the tables should be set up with horizontal rulings every five lines. This change Urban himself recommended after the issue of the *Praxis*. Both the tables in the *Praxis* and in Brown and Thomson have to be ruled by pen if they are to be put to much practical service.

E.G.B.

EXPERIMENTAL PSYCHOLOGY IN THE TALMUD

The following tale from the Babylonian Talmud[1] shows (if it be true!) that the method of expression and the use of the plethysmograph would have been understood by a psychological audience of the first century of our era. The tale is told of Vespasian, who is conversing with Rabbi Johanan ben Zakkai.

"In the meantime there came to him a messenger from Rome and spake to him saying: Up, for the Caesar is dead, and the nobles of Rome have agreed together to choose thee for their Caesar. Now he had at that time drawn on one of his shoes, and was in act to draw on the other, yet would not his foot pass therein; then he set himself to pull off that which he had drawn on, but it clave to his foot. Thereupon spake Johanan to him and said: Be not disquieted! Thou hast received good tidings, and it is written: A good report maketh the bones fat.[2]—What, then, said he, must I do?—And he said: Make to pass before thee some man that thou hatest; for it is written: A broken spirit drieth the bones.[3]—Then he did as Johanan had counselled, and his foot passed into the shoe."

Cornell University S. FELDMAN

THE MAX KLINGER BUST OF WUNDT

Dr. Walter N. Niles, of the Carnegie Nutrition Laboratory, has been good enough to inform me that a photographic reproduction of the Klinger bust, to which I referred in this JOURNAL, xxxii., 1921, 177, is issued as no. 225 of the postcard series of the Museum der bildenden Künste zu Leipzig (Verlag von E. A. Seemann in Leipzig). The pose is that of the lecturing Wundt: the head is thrown back rigidly, and the moustache is lifted above the lower lip, in a way characteristic of Wundt on the platform but unnatural to him in any other situation. The whole head is magnificently conceived, and the longer one lives with it the more certainly can one read parts and phases of the familiar Wundt out of it. It is, however, not a portrait bust.

E.B.T.

---

[1] *Gittin*, 56 *b*; L. Goldschmidt, *Der babylonische Talmud*, v, 1912, 546. This redaction of the Talmud was completed c. A. D. 500.

[2] *Proverbs*, xv, 30.

[3] *Ibid.*, xvii, 22. These two sayings, which themselves contain the gist of the whole matter, belong to the earliest collection of Proverbs, the accepted date of which is not later than B. C. 250.

BY EDWARD BRADFORD TITCHENER

D.Sc., Ph.D., LL.D., Litt.D., Sage Professor of Psychology in the Graduate School, Lecturer on Psychology in the College of Arts and Sciences, Head of the Department of Psychology in Cornell University

An Outline of Psychology (first published 1896) 1921 ..... $2.25
A Primer of Psychology (1898) ..... Out of Print
Experimental Psychology: A Manual of Laboratory Practice
I. Qualitative Experiments
Students' Manual (1901) 1919 ..... 2.60
Instructor's Manual (1901) 1918 ..... 3.50
II. Quantitative Experiments
Students' Manual (1905) 1915 ..... 2.60
Instructor's Manual (1905) 1915 ..... 3.50
Lectures on the Elementary Psychology of Feeling and Attention (1908) 1918 ..... 2.00
Lectures on the Experimental Psychology of the Thought-Processes (1909) 1.90
A Text-book of Psychology (1910) 1921 ..... 2.25
A Beginner's Psychology (1915) 1920 ..... 1.60
Systematic Psychology (*preparing*)

TRANSLATIONS

O. Külpe, Outlines of Psychology, Based upon the Results of Experimental Investigation (1895) $4.00
O. Külpe, Introduction to Philosophy (1897): translated by W. B. Pillsbury and E. B. Titchener 2.10
W. Wundt, Lectures on Human and Animal Psychology (1894): translated by J. E. Creighton and E. B. Titchener 3.50
W. Wundt, Ethics: An Investigation of the Facts and Laws of the Moral Life
Vol. I.: The Facts of the Moral Life (1897): translated by J. H. Gulliver and E. B. Titchener 2.50
Vol. II.: Ethical Systems (1897): translated by M. F. Washburn 2.50
Vol. III.: The Principles of Morality and the Departments of the Moral Life (1901): translated by M. F. Washburn 2.50
W. Wundt, Principles of Physiological Psychology
Vol. I.: Introduction: The Bodily Substrate of the Mental Life (1904) 4.00

THE MACMILLAN COMPANY
Publishers 64-66 Fifth Avenue New York

CORNELL UNIVERSITY

Courses of Instruction Offered by the Department of Psychology for the Academic Year 1922-23

1. **Elementary Psychology.** First term. Professor Titchener, Professor Weld, Assistant Professor Hoisington, Dr. Bishop and Mr. Feldman.

1a. **Elementary Psychology.** First term. Assistant Professor Dallenbach and Mr. Nafe.

1b. **Elementary Psychology.** Second term. Assistant Professor Hoisington and Mr. Feldman.

2. **General Psychology. Problems and Points of View.** Second term. Prerequisite, course 1, 1a, or 1b. Professor Weld.

3. **Experimental Psychology: Qualitative.** Repeated in second term. Prerequisite, course 1, 1a, or 1b. Professor Weld, Assistant Professor Dallenbach, Assistant Professor Hoisington, Dr. Bishop, and Mr. Nafe.

4. **Experimental Psychology: Quantitative.** Repeated in second term. Prerequisite, courses, 1, 1a, or 1b, and 3. Professor Weld, Assistant Professor Dallenbach, Assistant Professor Hoisington, Dr. Bishop, and Mr. Feldman.

[5. **Systematic Psychology: Sensation, Image.** First term.] Not given in 1922-1923.

5a. **Systematic Psychology: Perception, Idea.** First term. Prere quisite, course 3, or by special permission course 2. Professor Weld and Dr. Bishop.

[6. **Systematic Psychology: Feeling, Attention, Action.** Second term.] Not given in 1922-1923.

6a. **Systematic Psychology: Memory, Imagination and Thought, Emotion and Volition.** Second term. Prerequisite, course 3, or by special permission course 2. Assistant Professor Dallenbach and Assistant Professor Hoisington.

7. **Reading of German Psychology.** First term. Professor Weld.

8. **Reading of French Psychology.** Second term. Professor Weld.

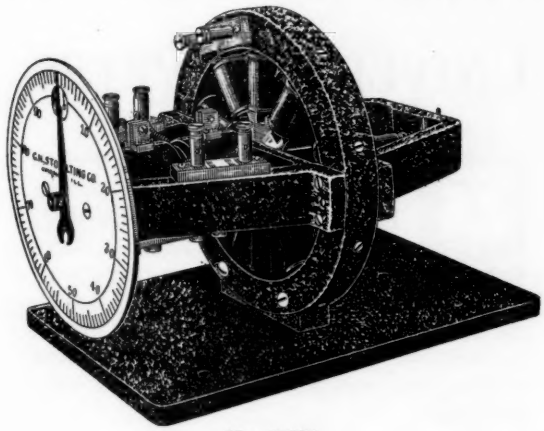
9. **Technique of the Laboratory.** Throughout the year. Dr. Bishop.

10. **Psychological Problems.** Throughout the year. Prerequisite, course 4. Professor Weld, Assistant Professor Dallenbach and Assistant Professor Hoisington.

11. **Research in Psychology.** Throughout the year. Professor Titchener.

12. **Seminary in Systematic Psychology.** Second term. Professor Titchener.

Dunlap Chronoscope



No. 20236

THIS popular, substantial and simple chronoscope is now being made in our establishment and carried in stock. Orders filled without delay.

The Dunlap chronoscope consists, as shown in the illustration, of a heavy iron base supporting a frame which carries a tenpole synchronous motor in the center; on one end the reaction and recording mechanism, and on the other the armature shaft with means for adjustment.

The motor is driven by a 25, 50 or 100 d.v. electrically maintained tuning fork. The tuning fork can be operated by the 110 or 120 volt direct or alternating current with the requisite accessories. The reaction mechanism must always be operated by a direct current.

The large stationary disk records fractions of a second; the small disk at the rear records seconds. The value of the large disk divisions varies with the rate of the tuning fork. With a 25 d.v. fork each division equals 4 sigma, with a 50 d.v. fork, 2 sigma and with a 100 d.v. fork 1 sigma. (See Prof. K. Dunlap's article in Psychobiology, Vol. I, No. 6, May 1918.) Further details of model II, also model III, furnished upon request.

No. 20236	Chronoscope (II) only	$200.00 net
No. 20237	Chronoscope (III) only	200.00 net
No. 23221	Master Key	11.00 net
No. 23223	Balanced Relay	32.00 net
No. 23224	Pneumatic Reaction Key	22.00 net
No. 17216	Voice Keys, per pair	72.00 net

C. H. STOELTING CO.

Manufacturers - Importers - Exporters

Psychological and Physiological Apparatus and Supplies

3037-3047 Carroll Ave. Chicago, Ill., U. S. A.

To Found a

UNIVERSITY PRESS

THE *Cornell Publications Printing Company*, printers of the 1922 volume of THE AMERICAN JOURNAL OF PSYCHOLOGY, were organized in the summer of 1921 for the purpose of providing funds for a university press at Cornell University. The profits from operations will be given to Cornell University for that purpose.

THE equipment is modern. The workmanship is intelligent. The organization is sufficiently flexible to enable an increase in capacity necessary to produce the printing that is attempted—*carefully, economically, and on time.*

THE service offered ranges from the printing of a few sample cards, or a single copy of resolutions, to the preparing of a publication. It can include editorial work, advertising make-up, and other service connected with printing and publishing business.

Cornell Publications Ptg. Co.

ITHACA, NEW YORK